Antiquity

A Quarterly Review of Archæology

Edited by O. G. S. Crawford, F.S.A.

JUNE 1930

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PRINTED BY JOHN BELLOWS, GLOUCESTER, ENGLAND

Price 5s. 6d—Annual Subscription 20s.

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ANTIQUITY is published quarterly on the 1st of March, June, September, and December.

THE ANNUAL SUBSCRIPTION (including postage) is ONE POUND (United States, 5 dollars). It is payable in advance to the ASSISTANT EDITOR, 24 Parkend Road, Gloucester, England.

SINGLE COPIES may be obtained through any bookseller for 5s. 6d., which includes postage. Trade orders should be addressed to the printer, John Bellows, Gloucester, England.

CLOTH CASES (lettered back and side) for binding Volume III (nos. 9–12) can be obtained from John Bellows, Gloucester, England, 4s. post free, or if the numbers are sent to him (post paid) the inclusive charge for case and binding will be 7s. 6d. Remittance must accompany the order.

MSS (whether in the form of Articles or Notes) offered for publication should be sent to the EDITOR, O. G. S. CRAWFORD, Nursling, Southampton, England, and a stamped addressed envelope enclosed. All MSS must be typewritten. Payment is made for Articles only. The EDITOR is not responsible for the opinions expressed by contributors.

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THE LONDON MERCURY, 229 Strand, London, W.C.2.

New Series] APRIL 1930. 2s. net. [Vol. XV, No. 57

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THE HISTORICAL ASSOCIATION

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Antiquity

A Quarterly Review of Archaeology

VOL. IV NO. 14 JUNE 1930

Editorial Notes

GOVERNMENTS do not often make generous grants of money in aid of archaeological research. Such grants as are made, however, are usually criticized for their meanness rather than for their extravagance. The South African Government has recently distinguished itself by making a grant of no less than £5000 to Dr Frobenius, who has been touring in South Africa with a large staff copying wall-paintings and endeavouring to solve the age and origin of the Rhodesian ruins. We do not know the exact conditions of the grant or what return the Government stipulated for; we only know that the grant has been made, or authorized. For the rest our knowledge is derived from press-cuttings of South African papers and from private correspondence with persons acquainted with the facts.

When we received the first press-cutting (from an American newspaper) we were incredulous. We recalled that Dr Frobenius had refused to accept the facts revealed by the British Association's excavations at Zimbabwe, both those of a quarter of a century ago (carried out by Dr Randall MacIver) and those of last year described by Miss Caton-Thompson in ANTIQUITY, December 1929. Both excavations were conducted most carefully and scientifically; the results of both were in agreement, and were the outcome of a number of observed facts capable of only a single interpretation—the recent age of the ruins. The wild suggestions put forward by irresponsible and unscientific theorizers had been put out of court and the question of the age of Zimbabwe could be no longer regarded as an open one.

Those who refused to face these facts could no longer be taken

seriously. For these reasons we hesitated to believe what the papers said and took steps to find out more. To our great surprise we found that report was correct.

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Apparently this new ' Maecenas ' (as the *Cape Argus* calls him in an excellent and witty leader of 25 February) is the Minister of the Interior, Dr Malan, to whom the good professor appears in the light of a ' sort of new Columbus ', opening up a ' new world that we did not realize existed before ' ! The Minister has only himself to blame for his ignorance of South Africa's archaeological treasures. The rock-paintings were published in book form (*Bushman Paintings*) as long ago as 1909 by Miss Helen Tongue, and facsimile reproductions are also exhibited in the Cape Town museum.

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The Editor of the *Cape Argus* is wholly justified in his ' uncomfortable feeling that Dr Malan has made himself and his colleagues in the Government more than a little ridiculous by throwing away his maxim of " South Africa first " in order to shower benefits on a foreign adventurer . . . Let us hope that when next he contemplates posturing as the Maecenas of archaeology he will resist the blandishments of peregrinating professors, or at least take independent advice from someone with a little knowledge and common sense'. We regard the grant as not only an insult to the archaeologists of Great Britain and South Africa, but also as a scandalous waste of public money. We only hope that it will not prejudice public opinion against further Government support for archaeology such as, if properly applied, would yield a rich harvest.

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There is one aspect of the Zimbabwe ruins that never ceases to amaze us ; namely, the fact that popular interest in the site should be lessened, or even cease altogether, if they are proved to be of native origin ! We suppose that it is to be accounted for on the *omne ignotum pro mirabile* principle. Surely the ruins should be infinitely more interesting if it is proved—as it may well be—that they are a ' home product ' ? Surely this is more to be proud of than the remains of imaginary Semitic adventurers ! From a strictly scientific point of view there can be no doubt whatever that Zimbabwe becomes *more* rather than *less* interesting, if it can be connected with other sites on the continent of Africa.

EDITORIAL NOTES

We are writing these notes in the middle of a spell of field-work. Readers of ANTIQUITY will hardly need to be told what field-work is, nor are we sure that we could tell them briefly if they asked us. We mention the fact merely to ventilate a need we have long felt—that of a catalogue of old manuscript maps and estate plans. Such maps exist in fairly large numbers in every county. Usually they are kept at the Estate Offices of large country houses, or, when such exist, in muniment rooms. They may be of any scale or age; a common scale is about 1 : 3000, and maps of an earlier date than 1600 are by no means common. For the field archaeologist they are absolutely invaluable. (So far as we are aware only one attempt has been made to catalogue such old maps). We do not know whether such documents come within the province of the Royal Commission on Historical Manuscripts; if not, we wish that some society or some private individual would calendar just those in a single county.

At the moment we feel the need of some such catalogue for Oxfordshire. Many of our problems would be solved by ancient field-names, and by pre-enclosure maps ; and the county is rich in old estates. But to hunt through all the Estate Offices one's self is an impossible task. The individual points that such maps determine are not, taken singly, of first-rate importance, though there are many exceptions. But field-work is essentially a mosaic whose pattern only becomes visible as the individual tesserae are restored to their right places. A beginning might well be made with the maps possessed by the colleges of Oxford and Cambridge. The task is a suitable one for post-graduate research and would be of more practical use than some theses, both to compiler and reader. We feel sure, moreover, that the authorities would willingly grant the necessary facilities.

The threatened quarrying for stone near Hadrian's Wall demands National action, and as we go to press we are glad to note that the matter is receiving Government attention. The proposals are not quite clear and until they are known exactly it is unwise to express views. Meanwhile public opinion has been aroused and we are content to await results.

Colonel Lindbergh's flight over British Honduras, the Peten region of Guatemala, and Yucatan, accompanied by two eminent field archaeologists, Drs Ricketson and Kidder, was an extremely interesting experiment.* It was hoped that aerial survey might prove of assistance to archaeological explorers in the Ancient Maya region, by determining the position of undiscovered ruins. The result is tantalizing. Certain, apparently new, sites were observed in the densely forested area, but it is quite clear that the observers were severely handicapped by the fact that they were flying over a country which is, for the most part, unmapped. It is significant that, in several cases, certain geographical features are quoted as ' probably ' this or that point. Information that a ruin has been discovered by air near a ' probable ' fixed point is not of assistance to an archaeological expedition which has to work its way thither on foot. In dense bush it is perfectly possible to pass an important complex of ruins within a hundred yards and see no trace of pyramids or buildings.

Under present conditions the position appears to be this :— An air-survey, over densely forested country, may reveal the existence of important archaeological sites, built on such a scale that their larger structures overtop the forest. But it cannot provide the accurate location which the party charged with the duty of developing the site, travelling on the ground, requires. The ground-party is necessary, because the aeroplane cannot land until a clearing is made. Once a landing place has been made, any excavation party would find its work facilitated to an almost magical degree, owing to the constant and rapid communication which it could maintain between camp and civilization. New Guinea furnishes an example; there the journey to the gold-fields can now be accomplished in forty-five minutes from the coast, whereas, by land, it takes about eight days.

It is obvious that air-transport and air-survey will provide and is providing enormous assistance to archaeological investigation in certain areas. But the flight under discussion, though interesting as an experiment, tends to show that a densely-forested and imperfectly mapped country must still rely on the earth-crawling party for its archaeological exploitation.

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* The results are published in the American *Geographical Review*, whose Editor has kindly supplied us with an advance copy.

Early Names of Britain*

by Eilert Ekwall
Professor of English, University of Lund, Sweden

THE earliest names of Britain known are Celtic. We know that the islands were inhabited before the coming of the Celts, but nothing about the language of the earlier inhabitants or the names by which they designated the islands.

Albion

This name is used by the Greek geographer Isidorus Characensis about the beginning of our era (Ἀλβίων), by Pliny (*Albion*), in an anonymous Greek tract, Περὶ κόσμου, formerly held to have been by Aristotle, but now shown to be much later and to date from the first century A.D. (Ἀλβιον), and by Ptolemy (Ἀλβίων). The form *Albion* is found in many later sources and is used by Bede. In literary usage it still lives on. Ptolemy also has a somewhat different form, Ἀλουίων, and the same form occurs in an anonymous Greek tract published in *Geographi Graeci minores*, ed. Müller, II, 497. The name *Albion* is always used of Great Britain, often in contradistinction to Ireland. A still earlier example of the name than those given is perhaps offered by the Latin writer Rufus Sextus Avienus (4th cent. A.D.), who in his *Ora maritima* mentions Great Britain under the name of *insula Albionum*. *Albiones* is the name of the inhabitants, presumably derived from *Albion*. Avienus is supposed to have used a now lost Greek work, perhaps by Eratosthenes (about 300 B.C.), and as he quotes the Carthaginian Himilco (about 500 B.C.) as his authority for the account of the British Isles, it has been suggested that the name *Albion* goes back to the Carthaginian.

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* The following notes, which are in substance identical with a lecture given to the Lund English Society a good many years ago, only claim to be a brief summary of the present stage of research in this field. For more detailed information I refer to Holder, *Alt-celtischer Sprachschatz*; McBain, *Etymology of the Principal Gaelic National Names*, etc. (Stirling, 1911); Sir J. Morris-Jones, *A Welsh Grammar*; Müllenhoff, *Deutsche Altertumskunde* I (Berlin, 1870); Rhys, *Celtic Britain*; Watson, *History of the Celtic Place-names of Scotland* (1926); Windisch, *Das keltische Britannien bis zu Kaiser Arthur* (Leipzig, 1912).

The meaning of *Albion* is generally taken to be ' white country ', the reference being to the white cliffs on the coasts, especially the south coast. It is a natural inference that the name was given by Continental Celts, very likely before the islands got a Celtic population. *Albion* is related to Latin *albus*, English *elf*, OE *ielfet* ' a swan ', the river-name *Elbe* and so on. The stem *alb-* ' white ' is not found in Celtic languages except in names such as *Alps* (earlier *Albes*), named from their snow-clad peaks, *Alba*, a common river-name, etc.

In a later form the name *Albion* has come to be used of the northern part of the island : of Scotland. In Irish the name developed forms such as Old Irish *Alpe*, *Albe*, Middle Irish *Alba*, *Albu*, genitive *Alban*. In Modern Gaelic the form is *Alba*, genitive *Albann*. In the earliest Irish sources the name is still applied to the whole island, but later it comes to be used in its now prevalent restricted meaning. I take it that this change in meaning is due to the migration of Gaels from Ireland to Scotland that took place in the fifth century A.D., when the kingdom of Dal Riada was founded. We may assume that after this event the most intimate relations between Ireland and Albion would be those between the Gaels in Scotland and the mother country. The name *Albu* would be used chiefly of the northern part, and this would easily cause the name to be restricted to that part altogether. The modern English *Albania* and Shakespeare's *Albany* represent the inflected forms (*Alban* gen., *Albain* dat.).

Britain

The name *Britannia* (*Britanni*, etc.) is mostly used only of the larger of the two British islands, and *Britanni*, *Brittones* of the inhabitants of the latter, but in early sources the two islands are sometimes called comprehensively ' the Britannic isles '. This latter usage is found in Polybius, the pseudo-Aristotelian tract, and Ptolemy. On the other hand already Strabo uses ἡ Πρεττανική of Great Britain in contradistinction to Ireland. It is impossible to determine definitely which usage is in reality the earlier. At any rate there is no improbability in the theory that *Britannia* originally denoted the larger of the islands and that the term Britannic Isles was formed later to designate both Great Britain and Ireland.

The modern name *Britain* is late French (Fr. *Bretagne*), but there was an OE *Breoten* (*Bryten*), which comes more directly from Latin *Britannia*, possibly through a Celtic medium.

Great Britain came into use to distinguish the mother country from *Little Britain* or Brittany, which was formerly often called *Britain* alone. In early sources it is not always clear if *Britain* (*Britannia*) means Great Britain or Brittany. In the fifth century A.D. a considerable number of Britons emigrated to Brittany in order to escape the invading Anglo-Saxons, and their new home was appropriately called *Britannia*. The distinction between the two Britains (*Britannia Magna* and *Parva*, or *Major* and *Minor*) was apparently not made until after the Norman Conquest. In the interesting paper by Mr D. MacRitchie and Mr W. H. Stevenson entitled *Great and Little Britain* (Society for Pure English, tract no. XVI), it is shown that Geoffrey of Monmouth seems to be the first who spoke of *Britannia Minor* in reference to Brittany; he uses *Britannia* alone of Great Britain. The term *Little* or *Less Britain* is afterwards used by English chroniclers, as Layamon, Robert of Gloucester, Robert of Brunne. The last two also speak of *the more Brutaine* or *Bretaygne þe grete* in reference to Great Britain.

The etymology and meaning of *Britanni*, *Britannia* and *Brittones* have given rise to a great deal of discussion. The whole problem is very complicated, not least owing to the curious variation found in the early forms of the names.

In Latin literature the most common form in the earliest sources is *Britannia* for the country, *Britanni* for the people. These forms are found in Catullus, Propertius, Vergil, Horace, Ovid, Pliny, etc. The metre shows that the *it* was short (*Britanni*,-*a*). The MSS of Caesar vary between *Britanni* and *Brittani*. Catullus appears to have *Brittania* by the side of *Britannia*. But on the whole the form *Brittani*(*a*) appears to be later. It is found in Frontinus, Ulpianus, Solinus, Ammianus and others. *Brittones* is likewise later, occurring in Martial and Juvenal.

In Greek sources the initial consonant varies between *p* and *b*. Polybius (2nd cent. B.C.) has Βρεττανικός, but we cannot, of course, be sure that the form of the MS is correct. The pseudo-Aristotelian tract (1st cent. A.D.) has Βρεττανικός, and similar forms (Βρεττανοί etc.) are given by Josephus (1st cent.), Ptolemy (or the MSS of his work), Dio, Appianus, Dionysius Periegetes (*c.* A.D. 300). But the form in *p*- is also very well evidenced. It is used by Strabo and Diodorus Siculus (about the beginning of our era), and there is reason to believe that it goes back to Pytheas. Ptolemy is stated by an ancient authority to have used the form. The form in *B*- most often has double *t* (Βρεττανικός), though a form with single *t* also occurs now and then. The form in

P- usually has *tt*, but Stephanus of Byzantium, who himself writes Βρεττανοί, remarks that Marcian and Ptolemy had Πρετανίδες νῆσοι.

Most scholars take the forms with *B-* and *P-* to be etymologically distinct. The etymology of *Britanni(a)* is generally held by these scholars to be obscure. Rhys, *Celtic Britain*, suggested that the stem might be that of Welsh *brethyn* ' cloth ', Irish *bratt* ' a cloak ', etc. If this is right, *Briton* would mean ' a person who wore clothes '. This etymology does not seem to have found acceptance and it may certainly be disregarded.

The other type (*Prettanoi*, etc.) is generally held to be connected with Old Welsh *Priten* (Middle Welsh *Pryden*) ' Picts ', Old Irish *Cruthen* ' Pict ', *Cruithne* ' Picts ', and Welsh *Ynys Prydain* (earlier *Prydein*) ' Britain '. Welsh *Pryden* goes back to **Pritenes*, and the stem *prit-*, like that of Irish *Cruthen*, is a still earlier Celtic **kurit-*. The different treatment of Ar. *kṷ* (British *p*, Irish-Gaelic *k*) is one of the chief means of distinguishing the two divisions of Celtic, British and Goidelic, and if *Prettanoi* has been correctly identified with the Celtic words for Picts, etc., and is not a later development of *Brettanoi*, we must assume that *Prettanoi* goes back to a British source and not to a Goidelic one. Personally I have no doubt whatever that the identification of *Prettanoi* and Welsh *Prydyn* is correct. The regular *e* of *Prettanoi* is probably due, as suggested by Sir J. Morris Jones, *Welsh Grammar*, §66, to substitution of Greek *e* for an open British *i*. I suppose the common double *t* is also due to inexact rendering of the British form.

It is generally held that *Pryden*, etc. is derived from Welsh *pryd*, Irish *cruth* ' form, figure, picture ' and means ' people adorned with figures, tattooed people '. The name would refer to the well-evidenced custom of the Picts of adorning their bodies with figures. According to Isidorus the Picts pricked their skins with needles and rubbed in the juice of plants. If this is right, *Pryden* would mean much the same as *Picts*, which is doubtless nothing but Latin *picti* ' painted ones '. This derivation is not accepted by Sir J. Morris Jones, who would rather connect the name with Welsh *pridd*, Irish *cré* ' loam ' and suggests that *Britannia* means ' the island of the white cliffs '. His objection to the usual derivation is that tattooing was not a characteristic of the Picts alone, but a common Celtic custom, so that the name ' tattooed people ' would be pointless.

This problem is a very difficult one, and I am not inclined to commit myself to a definite opinion. It is bound up, among other

things, with the question of the ethnological position of the Picts. But I should be loth to abandon the old theory, which permits us to derive *Pryden*, etc. from an actually evidenced Celtic word. As regards the force of the name 'tattooed people', it should be borne in mind that what Caesar says about tattooing among the Britons is only that they painted themselves with woad so as to look terrible in war. There is a difference between this temporary use of war-paint and tattooing, and 'tattooed people' would be a sufficiently distinguishing name for people who had the Pictish custom. It would not necessarily follow that *Pryden* was originally the name of non-Celtic aborigines of Britain. It is also conceivable that the name was applied to the Celts who first came over to Britain and may there have adopted the custom of tattooing from the aborigines. On the other hand there would be nothing improbable in the suggestion that *Pryden* (*Pritenes*) was first applied by Celts to non-Celtic aborigines, and that the name was later transferred to British Celts. Such transference of names is by no means uncommon. We have a good analogy in the Old English *Defnas* 'the Saxons in Devon', which is nothing but *Dumnonii*, the name of the British tribes in the district.

Sir J. Morris Jones believes that the names *Britanni* and *Prettanoi* are etymologically identical, and I have no doubt he is right. It would be too remarkable a coincidence if there should have been two sets of names so similar in form and with the same application, and yet etymologically distinct. He assumes that the form in *P-* is the earlier and that the form in *B-* developed from it. I am more inclined to believe that *B-* is due to inexact rendering of the Celtic form by Carthaginians or Greeks.

I have the impression that the names *Britanni*, *Brittones*, etc. are chiefly used by classical writers, and that they do not seem to have been much used by the Celts themselves. The Middle Welsh *Brytaen* 'Britain' is clearly an adaptation of Latin *Britannia*. Welsh *Brython* certainly occurs in early poetry, but has left only very slight traces. *Brethonec* is the Cornish name for the British language, and *Brezonek* the Breton name for the Breton language. But there is no formal objection to taking these to be derived from Latin *Brittones*. Latin *tt* becomes British *th*, as in Welsh *llythyr* from *littera*, etc. If this is right we should have to assume that the form with *tt* (*Brittani*(*a*), *Brittones*, etc.), which Sir J. Morris Jones takes to be due to hypocoristic doubling, developed in Latin.

ENGLAND

This is, of course, OE *Englaland* ' the country of the Angles '. The origin of the name *Angles*, OE *Engle*, is not definitely settled. Bede says the Angles came from a district called *Angulus*, which is universally identified with *Angeln* in Schleswig. Presumably the district was named from the deep narrow inlet of Schlei, and the name is related to Norwegian *angr* ' a narrow fiord '. Exactly the same name is found, in the form *Ongull*, as the old designation of a district in Norway, situated on a bay. Others would rather take *Angel* to have meant ' the nook or corner ' and to refer to the corner between the Schlei and the Flensburg fiord. The most generally accepted opinion is that OE *Engle* is derived from the district name *Angel* and means ' the people of *Angel* '. Against this it has been objected, however, that the Continental *Anglii*, according to classical authorities, did not live on the Baltic, but farther inland on the Elbe, and also that the small Angel district cannot well have been the home of the powerful Anglian tribes that settled in England. Professor Axel Erdmann, *Über die Heimat und den Namen der Angeln* (Uppsala, 1897), suggested that *Anglii*, OE *Engle*, was derived from a word meaning ' a spear '. A Danish scholar, Jessen, has suggested that the name developed in England after the Anglian emigration, that the ' corner ' that extends into the sea between the Thames and the Wash was once called *Angel*, the people receiving the name *Angle*. Recently my fellow-countryman, Professor Elis Wadstein, *On the origin of the English* (Uppsala, 1927), has modified this theory and suggested that ' *Angle* may be a Teutonic translation of *Iceni*, the name of the Celtic inhabitants of the country later occupied by the East-Angles '. He suggests that *Iceni* may have been combined with Celtic **īcen* (*īcin*) ' angle (hamus) ' : Breton *igen*, *higen* ' hameçon ' etc. and rendered in the language of the British Teutons by *Angili* or *Anguli*, etc. ; cf. Old English *angel*, *angil*, *angul* ' angle, fish-hook '. Whatever may have been the original home of the *Anglii*, I have no doubt myself that the English Angles (*Engle*) belonged to the old Teutonic tribe. And Bede may very well be fundamentally correct, even if the Anglii in the 1st cent. lived on the Elbe, for the *Anglii* may have originally lived in Angeln and, after migrating southwards and absorbing other tribes, have gone over to Britain.

The name *Anglians*, *Engle*, at an early date began to be used with reference to all the Germanic tribes in England. Already Alfred the Great, of the Wessex house, calls England *Angelþeod* and the language *Englisc*. But the Celts to this day use the word *Saxon* for English(man).

Sais is Welsh for 'Englishman'; it comes from Latin *Saxo*. A Welsh name for England is the enigmatic *Loegr*. The Irish name for England is *Sa(c)san* or *Sasana*, for Englishman *Sa(c)sanach*. This is easy to understand, for the first 'Anglo-Saxons' with whom the Celts came into contact were the Saxon pirates who began to harry the British coasts long before the Anglo-Saxon invasion took place.

A few notes may be added on the names of Wales, Ireland, Scotland.

WALES

This is an English name, really a tribal name, OE *Wealas* 'the Welsh'. It is a common phenomenon that tribal names become names of countries or districts. In England there are among others *Essex*, *Middlesex*, *Sussex*, *Wessex*, respectively 'the East, Middle, South and West Saxons'. The German *Baiern*, *Franken*, *Schwaben* mean 'the Bavarians, Franconians, Swabians'. *Wealas* is the plural of *Wealh*, and *Welsh* (OE *Wielisc*) is a derivative of the word. *Wealh* itself is an early loan-word from Celtic. It represents a modification of *Volcae*, the name of a Celtic tribe with which the Germans came early into contact and whose name they transferred to other Celts. The same word enters into *Cornwall*, OE *Cornwealas*, which is again a tribal name and means 'the Cornish Welsh'. *Corn* commemorates the Celtic tribe of the *Cornovii*. The Welsh name of Cornwall is *Cernyw* (Latinized *Cornubia*). The Cornishmen themselves called their country *Kernow*. *Cornovii* is derived from Celtic *cornu-* (Welsh *corn*) 'a horn' and means 'the people on the promontory'.

The Welsh call themselves *Cymry* and their country *Cymru*. *Cymro* 'a Welshman' (from **kom-brog*) means 'compatriot'. The name is supposed to have arisen after the Anglo-Saxon invasion. It was applied not only to the Britons in Wales, but also to those in the north of England and Scotland, who fought together against the invader. *Cymry* became OE *Cumbras*, and this name survives as the first element of *Cumberland*, where the Britons held their own for a long time.

IRELAND

Ireland, OE *Iraland*, means 'the country of the Irish'. OE *Iras* is derived from the old name of Ireland, old Irish *Hériu*, *Ériu*, gen. *Hérenn*, now *Eire*, gen. *Eireann*, Gaelic *Eireann*. The Welsh form is *Iwerddon*. English *Erin* represents the inflected form. *Ériu* comes from earlier

**Iveriō*, which appears in classical sources as Ἰέρνη Strabo, etc., Latin *Iuverna*, *Hibernia*, etc. In Celtic languages Aryan *p* disappears (cf. Old Irish *athir* = Latin *pater*), and we may assume as a still earlier form **Piveriō*, a form which is indeed supported by the initial *H-* of the earliest Irish form. An initial *h-* occurs sometimes in early Irish as the last rest of a *p*. *Piveriō* has been convincingly compared with Greek Πιερία, the name of a district in Greece, the home of the Muses. Both belong to the root *pi-* ' fat ', etc. found in English *fat*, etc. The meaning of *Erin* would thus be ' the fertile country '.

Scotland

The inhabitants of Ireland were known as Gaels, earlier Gaedels or Goidels, a name of unknown etymology. Another name was *Scots*, Latin *Scotti*, and Ireland is sometimes referred to as *Scottia*. The meaning of *Scot* is also obscure. After the Irish emigration to Scotland the name *Scots* was applied also to the Gaels in Scotland, and eventually it was restricted to the Scottish Gaels. From the tribal name (OE *Scottas*) was formed the Old English name *Scotland*, which is used by Alfred the Great in *Orosius* of Ireland, but in the *Anglo-Saxon Chronicle* under the year 933 is applied to Scotland.

A name that is sometimes used of Scotland is *Caledonia*, which strictly refers only to the northern part. This name is derived from *Caledones* or *Caledonii*, the name of a powerful British tribe settled in the north of Scotland and often mentioned by classical writers. The name is still preserved in *Dunkeld*. The etymology of the name is disputed. The old derivation from the root of Welsh *celli* ' wood ' cannot be upheld. Welsh *caled* ' hard ' comes from *calet* and cannot be the immediate source, but it may be a related word.

The Sword-bearers

by Estyn Evans

EIGHT years ago, in an article entitled 'A prehistoric invasion of England',[1] Mr O. G. S. Crawford put forward the hypothesis that 'towards the close of the Bronze Age the British Isles were invaded by the first wave of Celtic-speaking peoples bringing with them leaf-shaped bronze swords, many other entirely new types of bronze objects, and at least two types of pottery new to these islands'. It may perhaps be said that this view, with certain qualifications, notably as regards chronology,* has met with general acceptance. A comparative study of types of bronze implements over a wide geographical field, while yielding corroborative evidence in support of the invasion theory, has also raised important problems in other directions; and it is my present object to give the results of an enquiry into the origins and distributions of certain type-specimens of the late Bronze Age cultures of western Europe.

In Britain, bronze itself appears to have been quite rare during the early and middle parts of the Bronze Age. Hoards of bronze implements are few in number; and it is not until the end of the period that the metal industry was fully developed. The change is marked by the appearance of the true winged celt or axe (plate 1), and by certain other 'exotic' objects often found associated with it in hoards. A similar disparity between the cultures of the middle and late Bronze Age has been noticed elsewhere—in Belgium, the Paris Basin, Brittany,† Spain and South France. In Picardy,[2] for example, the industries are very monotonous until the appearance of the winged celt, associated with which one invariably finds numerous artifacts and ornaments of a new design and skilful execution. Many independent workers have commented on the general similarity (in some cases amounting to

* The date suggested was 800-700 B.C. Some workers would place the main invasion two or three centuries earlier while others would bring the date forward by a similar period.

† Here the cult of the square-socketed axe is equally distinctive, but this problem cannot be pursued here.

identity) between the objects found in such complex hoards* and those represented in every collection of ' lake-dwelling material '. The highly characteristic winged celt, whether found in Spain, Brittany, Picardy or Essex, is precisely the *lappenabsatzbeil* which forms 70 per cent. of the total varieties yielded by the pile-dwellings of Switzerland and Savoy.

In Britain the distribution of axes of this type (fig. 1) is very restricted, while in France there are marked concentrations along the coastal regions between the Loire and the Somme. Scattered finds occur along the river-lines of Loire, Seine, Marne and Moselle, radiating from the west-Alpine culture province, the most characteristic centre of the winged axe.[3]

Many type-objects of this culture have been noticed by Crawford, who is dealing specifically with Britain. An important link is provided by the tanged two-edged razors, widely distributed among the hoards of northwest France. A small rectangular blade,‡ perforated near the back, has been found in Essex and Kent, and in the French departments of Morbihan, Manche, Finistère, Cher, Ile-et-Vilaine and Charente. The socketed dagger (or knife) is another type-object. Of special interest are the pins of the type of the lake-dwellings, notably the thistle-headed variety so common in the west-Alpine province and found in the departments of Jura, Oise, Cher, Morbihan and Seine-et-Oise. We may further mention the curved-back knife of the lake-dwellings, socketed celts of certain forms—frequently with vestigial wing decoration—rings, and the socketed types of hammers, sickles, gouges and chisels. There can be little doubt that, although many of these bronzes originated farther east, it was from the west-Alpine province that they spread to Britain. But more conclusive evidence is forthcoming from a study of the bronze sword.

It might be claimed that certain objects and customs were introduced into this country by the ' peaceful penetration ' of trade rather than by invading hosts of immigrants. But, while fully admitting that such influences were at work, I think it will be generally agreed that nothing short of actual immigration can account for the spread of new and special forms of a ' superior weapon ' such as the bronze sword. Moreover, the evidence is cumulative; and when considered as a whole, the case for invasion is a very strong one.

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* See Appendix.

‡ The type-example is that from the All Hallows hoard (Hoo, Kent). Evans, *Bronze Implements*, fig. 261.

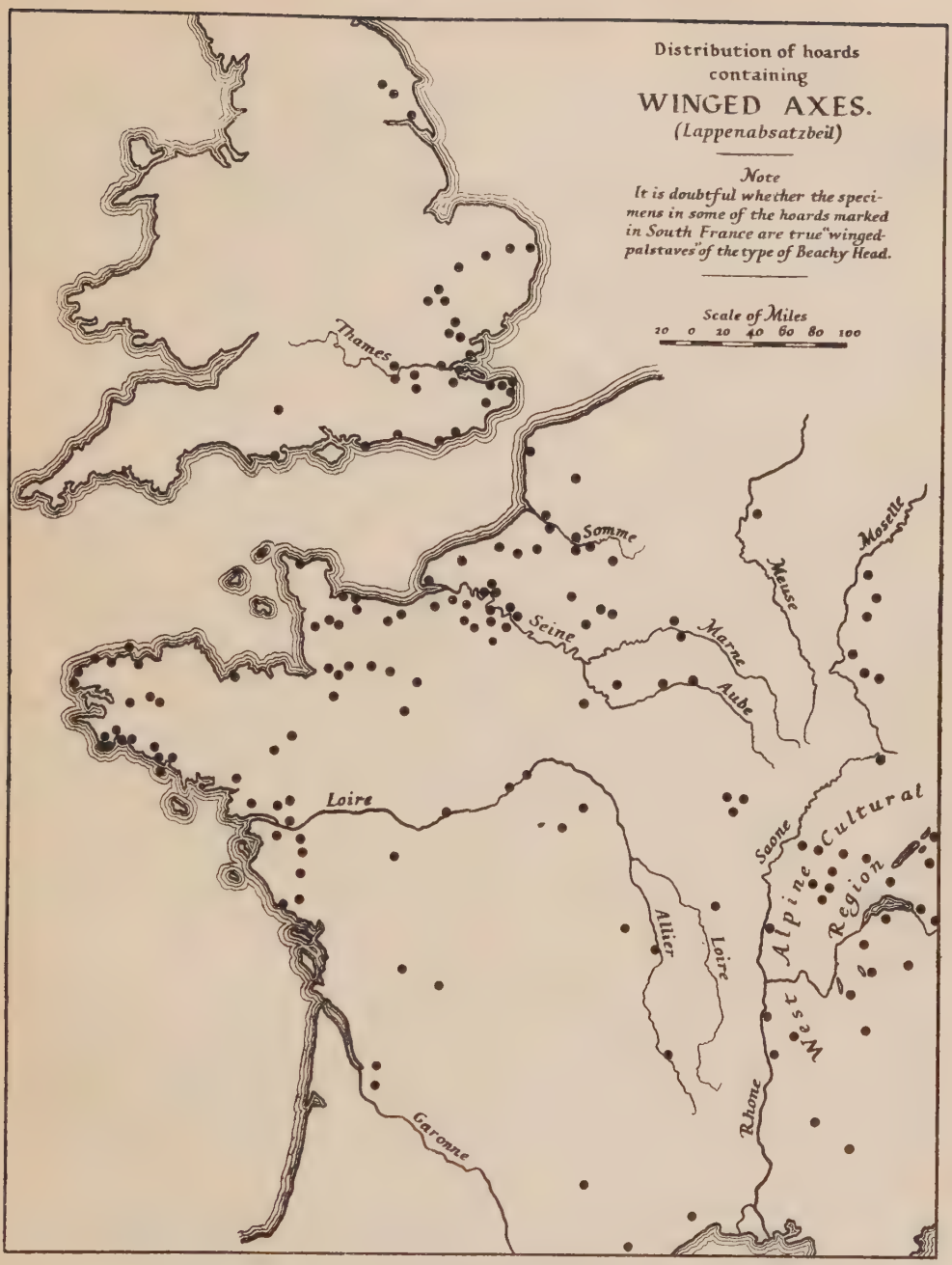
Distribution of hoards
containing
WINGED AXES.
(Lappenabsatzbeil)
Note
It is doubtful whether the specimens in some of the hoards marked in South France are true "winged-palstaves" of the type of Beachy Head.
Scale of Miles
20 0 20 40 60 80 100
Thames
Somme
Seine
Marne
Aube
Meuse
Moselle
Loire
Allier
Loire
Saone
Cultural
Alpine
Region
West
Rhone
Garonne

FIG. 1

Several attempts have been made to connect British types of sword with invasive movements of the late Bronze Age ; but as yet no serious notice has been taken of a sword which is associated, in nearly every case, with the more complex hoards of France and southeast England. That this important culture-link has escaped attention may be attributed to the fact that attractive unbroken specimens are rare—they are unknown in the large hoards, occurring only in isolation ; but the map (fig. 2) will serve to indicate its capital importance for the problem of Britain's continental relations in the late Bronze Age. Not only is this sword of frequent occurrence in the complex hoards of France and lowland England, but its distribution area is sensibly the same as that of the winged celt. It does not occur east of the Rhine* or south of the Alps. Brewis recognizes it in seven founders' hoards in Sussex, Surrey, Essex and Kent, and in a fine example from the Thames at Brentford (plate II). The blade differs from that of the usual leaf-shaped sword in having parallel, or almost parallel, sides and frequently a narrowed termination of the kind called ' carp's tongue '. It has a rounded mid-rib outlined at each side by incised lines that ' sweep outward at the *ricasso* following the line of its curved edge '.[4] (Plate IV, no. 3). Very characteristic are the deep squarish notches at the base of the blade, which provide, in conjunction with the broad tang, a rudimentary guard unknown in the leaf-shaped striking swords. The curve between tang and butt tends to be slightly concave, and thus the sword falls late in the scheme of classification proposed by Peake.[5] Examples of hilts from various localities are shown in fig. 4. It will be seen that, neglecting minor variations in the method of attaching the hilt plates, the swords have essentially the same features and must be classified apart from the broad striking swords which have hitherto virtually monopolized attention.

Passing over the details for France and Britain, we may turn to the Iberian peninsula (not included in the map). It has been stated that flange-hilted bronze swords are absent in the peninsula. This may be true of specifically *leaf-shaped* forms, but the type described above is by no means uncommon, and its unquestionable associations at Huelva,[6] where 75 specimens were found, provide a valuable clue

* The only exceptions appear to be two specimens in the British Museum, one labelled ' Denmark ' and the other (a variant) from Saxony. Naue's plates in *Die vorrömischen Schwerter* include no central European examples : indeed the only specimen shown is from Vénat (Charente).

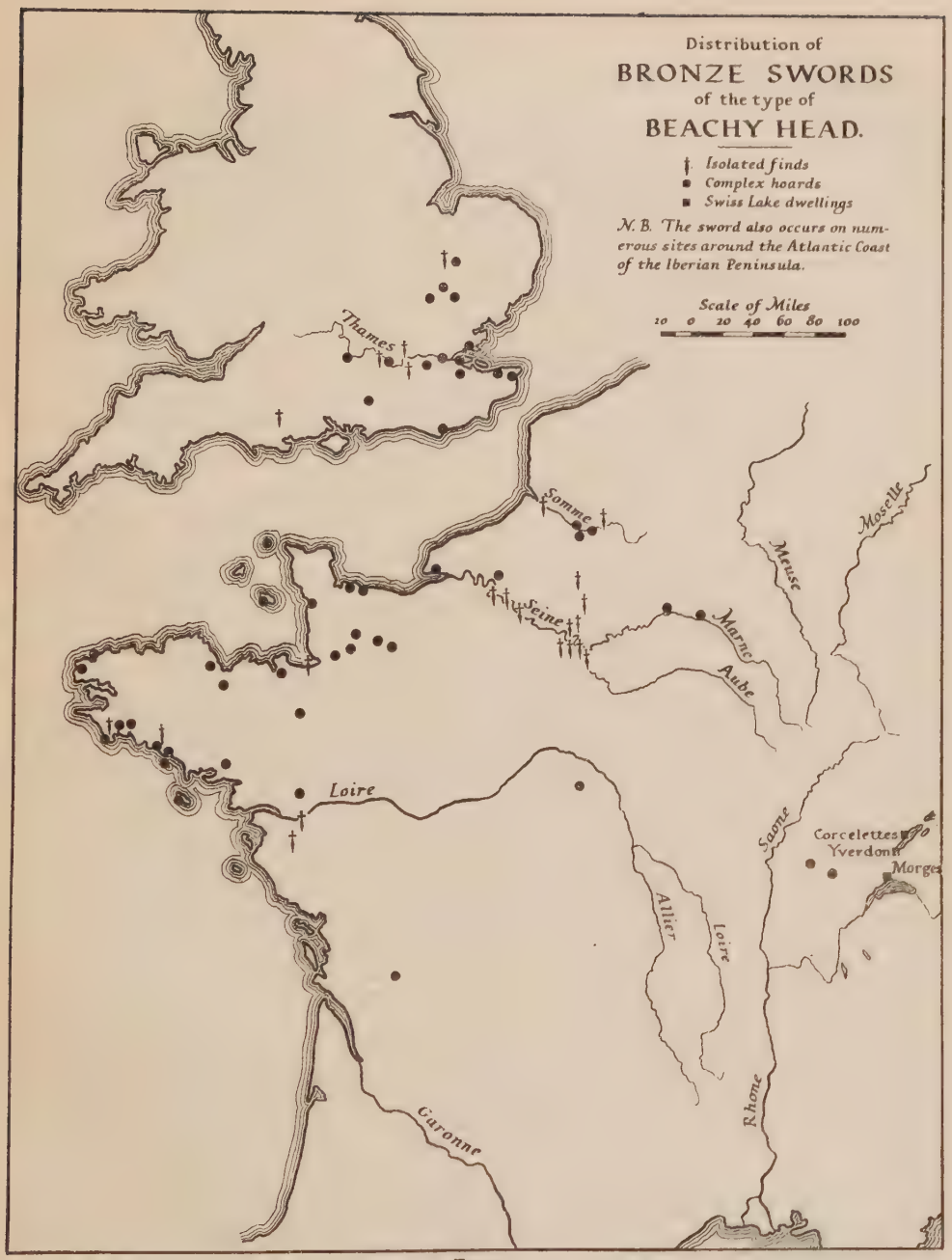

FIG. 2

This map shows only those hoards where it has been possible to check the type of sword: in all probability there are scores of other sites in certain regions, especially Brittany. The three pile-dwellings marked are those which have yielded swords.

to its chronology. The British Museum has a specimen with carp's tongue point from Tabernas (Almeria). Proved associations are rare in the peninsula, though Bosch-Gimpera states that the sword prevails throughout the Asturias, Galicia and Portugal ' with axes of Western European type '.[7] Several examples from Estremadura and Alemtejo are to be found in the Ethnological Museum at Belem, Lisbon :* one hilt from the hoard of Carvalhal (Estremadura Cistagana) is the exact counterpart of specimens from Vénat (Charente) and Questembert (Morbihan). There are thus clear indications of fragmentary intrusions of this northern culture around all the coasts of the peninsula, the Mediterranean coast alone being excepted.

It might be thought that we are dealing with diffusion along the Atlantic coastal route, and in all probability that is partly true ; but the interest of this distribution increases when we find that the thrusting sword also occurs in the Swiss pile-dwellings and in the Jura. It is possible, further, to follow its evolution there. We have seen that a considerable body of evidence points to the west-Alpine cultural province as the source of the winged axe and of the associated bronzes that appear in fixed forms around the coasts of France and England ; and this culture may now claim our attention.

Modern investigations have shown that the large pile-dwellings which have yielded the rich museum medleys generally labelled ' lake-dwelling material ' belong to a late and distinct phase in the habitation of the lake shores.† There is little doubt that the high-grade industry of this period was based on a degree of agricultural prosperity previously unknown. To the same period of optimum climatic conditions north of the Alps belong the splendid artistic achievements of the Scandinavian and eastern Hungarian Bronze Ages. Around the lakes the dried shores would seem to have been utilized for agriculture, and it is suggestive that vast numbers of bronze sickles have been found in this region of the western Alps.

This strongly unified west-Alpine culture, best exemplified in the pile-dwellings of Switzerland and Savoy, owed its virility to the grafting of intrusive east-Alpine culture-traits on to a conservative native tradition which produced, throughout prehistoric times, ' great unitary

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* I am indebted to Miss L. F. Chitty for allowing me to make use of her notes on the collections at Belem.

† See Dr Viollier, in *Pfahlbauten : zehnter Bericht*. Zürich, 1924. [Reviewed in ANTIQUITY, 1927, 1, 381-3].

cultures in the upper course of the Rhône and its tributaries, the Saône and the Doubs '.[8] The revived cultural focus of the Rhône was henceforth to play, until far on into the Iron Age, a predominant rôle in the diffusion of the arts and crafts throughout western Europe. Artistically the region is marked by a restrained style of decoration consisting of rigidly organized line-ornamentation. 'Perfect elegance, combined with an obstinate conservatism in shapes, is the characteristic feature of this art as a whole'; and this is strikingly true of the bronzes enumerated above.

It has long been clear that the west-Alpine cultures played a leading part in the evolution of later varieties of the bronze sword. Peake thinks that his 'proto-Hallstatt' form (F)[9] originated in Switzerland, but I am inclined to regard this as a type peculiar to the British Isles and to see in his type E the variety from which the Hallstatt weapon evolved.

The urn-field culture (*Oberendingen*)[10] was responsible for the introduction into the west of the first flange-hilted sword; and from this prototype (Peake's D, dated, from its occurrence at Mycenae and in Egypt, to the end of the 13th century), certain regional types, varying according to local artistic tradition, were developed around the Alpine borders. Thus it was in the territory adjoining Switzerland on the east—the Illyrian province of Kraft—that the iron sword ultimately appeared, modelled on the bronze striking sword that retained the leaf-shaped blade of type E. But in the Rhône province a different regional tradition* perpetuated the straight blade of the forms under discussion, and ultimately produced the solid hafted swords of the types of Mörigen and Auvernier. The hafts in plate III illustrate the tendencies of development from a form (no. 1) which has some of the features of the base type D. These tendencies are :—the deepening of the notches, the incurving and flattening of the hilt-plate, reduction of flanges, and with that a widening of the haft that recalls the development of the bronze Hallstatt sword. When the custom of casting the haft in solid bronze became general (probably under the influence of South Germany), the straight blade was preserved and the incipient guard was accentuated in the Rhône valley (Mörigen) sword.

* The rapiers of the Mels-Rixheim culture (of Italian affinities) may have played some part : the blade with its rounded mid-rib tends to end in a narrowed point, and the true carp's tongue is not uncommon in Italian swords. The thrusting weapon, it has been observed, has always been more in favour among Mediterranean peoples than the striking sword of the North.

We may briefly notice some characteristics of other bronzes of the west-Alpine province. The invention of the socketed celt was one of the contributions of the Lausitz culture-centre ;[11] and it was in the application of the idea of the socket that the craftsmen of the west-Alpine area excelled. The skilful use of core-casting was maintained and even improved by the bronze-workers who settled on the Atlantic coasts, and it is well illustrated in the bugle-shaped objects (plate IV, no. 1) found in several of the complex hoards of France and Britain and variously described as chapes, buckles, horse-trappings and ' mysterious objects '.[12] Its associations suggest that it was used as a fastener for a sword belt. The persistence of its intricate shape and its distribution (fig. 3) are alike significant. Though there seems to be no example of this object among pile-dwelling collections, a very frequent instrument there is a bronze loop of similar shape but cast solid (plate III, nos. 4 and 5), doubtless the prototype of the hollow fastener. The solid form also occurs in France, as at Frouard (Meurthe-et-Moselle).

It must be borne in mind that the Jura formed part of the west-Alpine province, and the importance of this favourable limestone tract, rich in salt supplies and therefore attractive to cereal growers, who seem universally to require salt with a cereal diet, has been frequently demonstrated. It was more easy of access in early times than either the Belfort gap to the north or the Swiss plateau to the south ; it served as a westward avenue for more than one penetration into France. When resistance had weakened with deteriorating climate in the first centuries of the last millennium, the bearers of the Hallstatt sword pushed west along this line, and traces of a refugee movement from the western Alps occur throughout the Midi, where hoards of late date have frequently been found in grottoes and rock shelters. Incidentally we may note that the ' industrie launacienne ' of Déchelette[13] probably represents a focus of Bronze Age survivals in Hérault, untouched by the early Hallstatt invaders.

Coutil[14] enumerates some two dozen hoards (mostly of late date) from the departments of Doubs and Jura, and in the latter department was discovered, in 1865, the important hoard of Larnaud. Most of the types—winged and socketed celts, chisels, swords, razors, buttons, bracelets, daggers, etc.—can be exactly matched in the hoards of northwest France and southeast England, while the fish-hooks found there provide another link between the lakes and the hoards of Oise.

The conclusion, therefore, is irresistible : there was, towards the end of the Bronze Age, a movement of peoples from the Rhône province

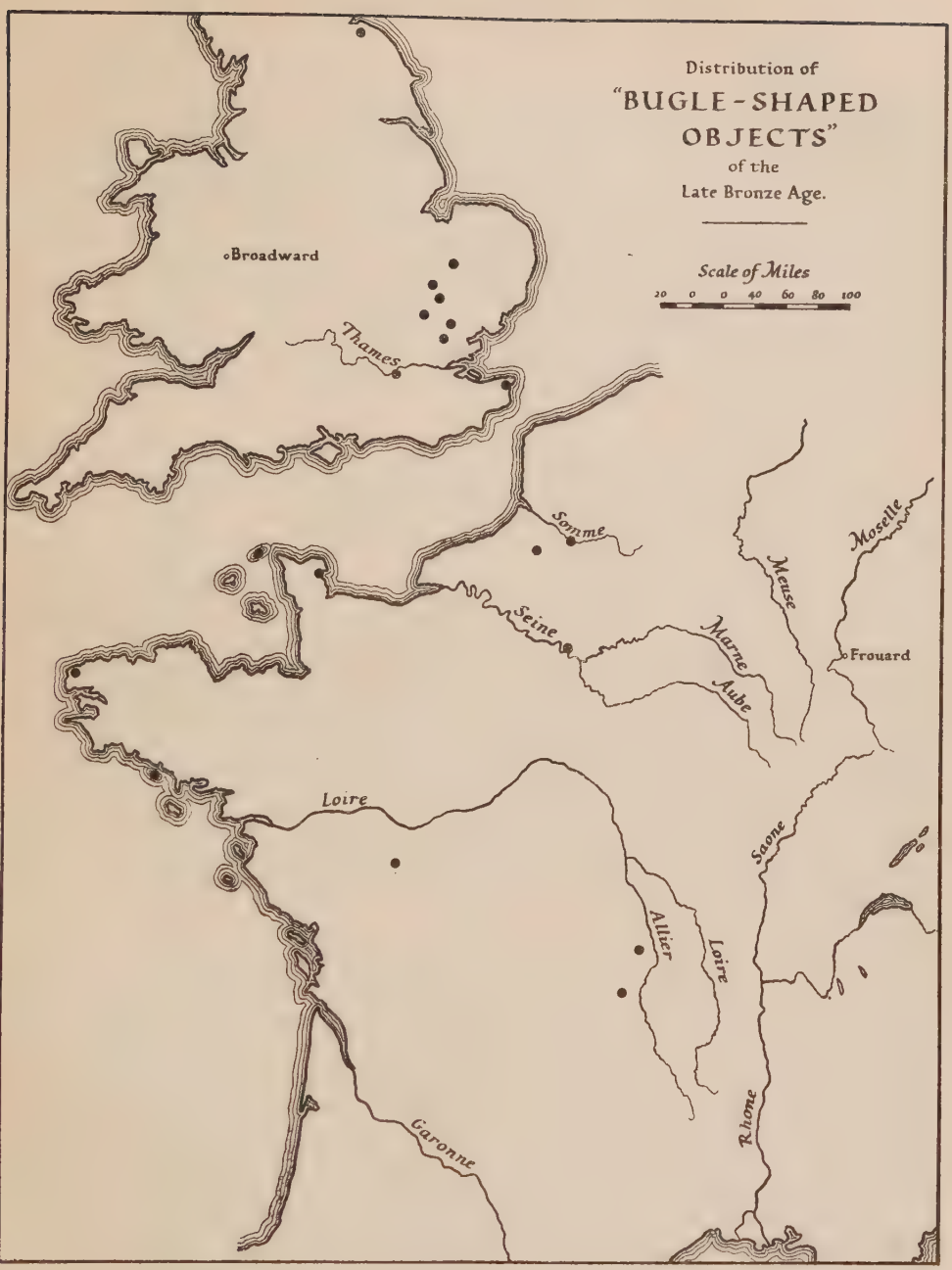
Distribution of
"BUGLE-SHAPED OBJECTS"
of the
Late Bronze Age.
Scale of Miles
20 0 0 40 60 80 100
Broadward
Thames
Somme
Seine
Moselle
Meuse
Marne
Aube
Frouard
Loire
Saone
Allier
Loire
Rhone
Garonne

FIG. 3

to the northwest. One must of course recognize the complexity of this movement, and that others of a peaceful, commercial kind were involved with it. Nevertheless I regard it as certain that an actual folk-movement did take place, and I would assign it to the end of the second millennium B.C. That there were other spreads (to Catalonia, for example) is demonstrated by Kraft,[15] but the movement to the southwest seems, from the evidence at present available, to have been distinct from that to the northwest. The same factor, however, was probably at work in both cases—expansion due to prosperity, and perhaps to pressure of population, during a period of marked progress in the adaptation of man to his physical environment in the region north of the Alps.

The vigorous west-Alpine culture was carried down the radiating river-ways of Loire, Seine, Marne and Meuse. We find little trace of the emigrants in the forested plains of central France ; but northwards they reached Belgium, though in no great force, and westwards their industries superseded the languishing native palstave-culture, and along the ancient coastwise routes they expanded rapidly. This movement probably represents the first wave of Celtic-speaking people which reached the peninsulas of the far west. Without embarking on controversial waters it is interesting in this connexion to recall a suggestion made by Sir John Rhys.[16] The potentialities of the Alpine region as a source of population, he remarks ' are instanced so late as the time of Caesar, when the Helvetii set out from their country to seek a home elsewhere, and though they were unsuccessful, many similar migrations had probably succeeded before '. It is worth recording that the Helvetii were making for the territory of the Santones in western Gaul.

Unfortunately we know next to nothing of the dwellings or the pottery of the invaders, at any rate on the continent, though, as in Britain, evidence would probably be found if sufficient search were made. The French hoards have frequently been found contained in ' de grands vases unis ou simplement ornés de cordons d'applique, avec impressions digitales ',[17] a description that reminds us of the urns (barrel- or bucket-shaped) with raised ornament which we find in Britain during the period of transition from bronze to iron. Though the origins of these urns are obscure they are undoubtedly exotic so far as Britain is concerned ; and the frequently associated Deverel pots can be traced definitely to the urnfield cultures of eastern France, being ultimately derived from Lausitz types.

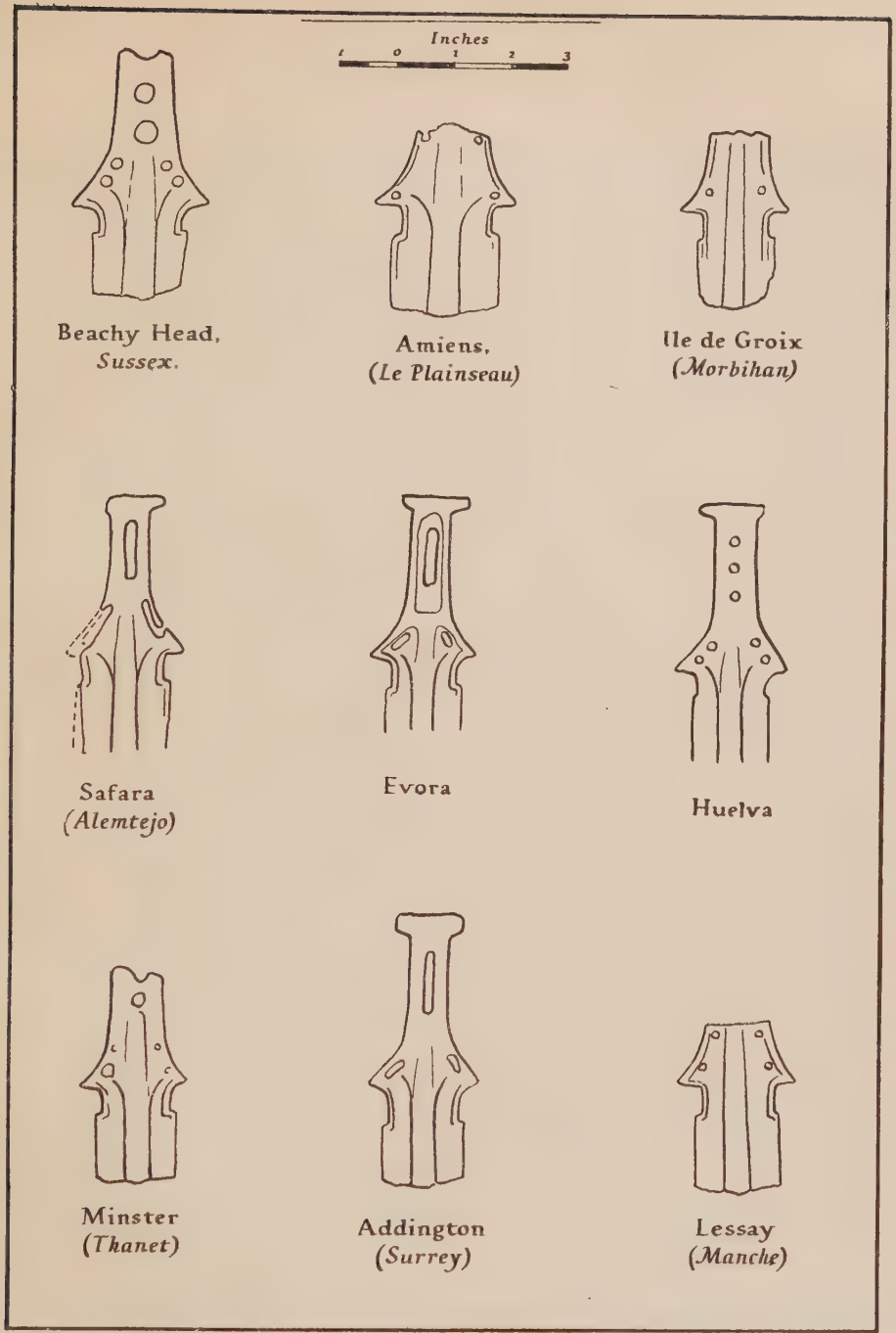

FIG. 4

SWORD-HILTS FROM SOUTHEAST BRITAIN, PICARDY, NORMANDY, BRITTANY, SPAIN AND PORTUGAL

Peake has claimed that the earlier forms of these urns were introduced in the pure Bronze Age by invading peoples armed with swords of type E; while he would associate the occasional Hallstatt culture-traits in south England with invaders who carried the bronze Hallstatt weapon. But this sword is far from common anywhere in northwest Europe, and there seems to be little indication of anything more than fragmentary intrusion of the culture of the *early* Hallstatt period. Nor is type E found, save sporadically, among the complex hoards marked on the distribution map (fig. 1). It probably reached Britain by trade during the commercial expansion of central Europe in the 12th century. Early forms are found in Eure and in the Somme area, in association with native palstaves. Later, from the northern foreland of the Alps, Mörigen and antennae swords found their way even to Finland and central Italy.

The E swords are found widely distributed in England and Wales, (though there is a marked group in the lower Thames valley), and they seem to have been manufactured locally from foreign models, and to have given rise to the characteristic leaf-shaped sword of the late Bronze Age in north and west Britain and in Ireland (type F).

We conclude, therefore, that it was with the thrusting sword of the type of Beachy Head that the first group of invaders landed around the continental coasts of England, somewhere about 1000 B.C.,* the first and not the least important of a long series of incursions that persisted until the coming of the Romans. The new comers probably brought with them improvements in the means of production, in social organization, and certainly in the art of metal working as revolutionary as any that our islands had previously known. They settled in the lowlands, often on offshore islands, as in Brittany, and seem to have penetrated inland along several lines, first of all, perhaps, by the south side of the

* It has been contended, particularly by Dr R. C. C. Clay, that the late bronze industries of Britain were introduced, together with the new types of urns, at a time, in the early Iron period, when the use of iron was already known. The argument is set forth in a paper on the Pokesdown urn-field (*Antiquaries Journal*, VII, October 1927), but the evidence for a long overlap is inconclusive and cannot be accepted by the student who approaches the problem from the chronological bases of the Bronze Age. The advanced Bronze Age must have had an independent life of some duration. There was, of course, some overlap in the use of the older metal: bronze working did not go entirely out of fashion on the introduction of a knowledge of iron. One would expect its survival for the construction of precisely those objects on which Dr Clay's argument is based: the socketed axe, which could not be cast in the new metal, and such articles as razors, rings, beads and other ornaments.

Thames. ' But their hearts would be set on the good things that they had heard to be awaiting them north of the river, the fat flat sunny cornlands of East Anglia, still the teeming mother of the best English wheat'.* This is an essayist's fancy, but archaeology supports this conjectural explanation of the *raison d'etre* of London. We find abundant traces of the invaders at Grays, where a natural pier of chalk and gravel invites a crossing, and at many other spots on the gradually narrowing estuary, until at Brentford we come to the easiest crossing of all ; and close by at Old England† their culture, with winged and socketed axes, finger-tip pottery, carp's tongue swords, belt-fasteners, razors, pile-dwelling pins, tweezers, chisels, and so forth, is found in unmistakable completeness.

The limited distribution of this industry must not be taken to indicate that it played no significant part in shaping the cultures of Britain. It is true that the finely curved winged celt and the thrusting sword did not spread : they were fixed forms adapted, perhaps, to particular needs. The socketed axe lived longer, though it too was the last of its line and did not long survive the introduction of iron weapons in the southeast. But in those parts of the British Isles remote from Continental influences the socketed axe had still a long life before it ; and in general the character of the late Bronze Age cultures of most of Britain and Ireland seems to have been determined by the absorption of elements derived from that of the invaders. The striking sword (E) had spread at an earlier date throughout Britain, and it was found more serviceable than the new weapon introduced by the Alpine villagers ; but many objects of various types, useful and ornamental—socketed knives, gouges and chisels, hammers, chapes, tweezers, razors, buttons, sickles, bracelets, pins—were received from the southeast, reaching Wales and Ireland about the 9th century, and persisting long after iron had come into general use in most of lowland Britain. The Llynfawr hoard on the margins between the two regions, provides an interesting example of the persistence of bronze types in iron.

The winged axe was not entirely without influence on the native forms : for palstaves with the flanges bent over in obvious imitation of wings occur in Wales and Scotland and are common in Ireland, especially

* C. E. Montague, *The Right Place*, p. 173.

† The importance of this site was demonstrated by Crawford in *Antiquaries Journal*, 1922, II, 33. See also R. E. M. Wheeler, in *Antiquity*, March 1929 (especially plates I and II).

in the northeast. In connexion with this and with similar phenomena we should recall the theory advanced by Dr Cyril Fox,[18] in working out cultural contrasts between highland and lowland Britain, that the encrusted urns of the former province (commonest in northeast Ireland and dated by Fox from 900 to 600 B.C.) represent the *absorption* of the culture which introduced urns with ornamentation on raised ribs into southern and eastern England. It is not unlikely that with this absorption, and without any important movement of peoples, the first Celtic language brought by the invaders from the Celtic Cradle may have reached the west, replacing a primitive tongue of pre-Aryan type. The new language would in this case have reached Ireland, like other intrusive elements at many periods, by way of the northeast.

Meanwhile fresh continental influences were coming to the south and east coasts of England with the manifold movements brought into existence by the iron sword and by the destruction of the pile-dwellings by floods about 800 B.C. It was their arrival that dealt the death-blow to the late Bronze Age in lowland Britain. At first the new comers may have landed at Christchurch, penetrating through the open gravel corridor of the lower Avon valley to Wiltshire; but the excavations on Park Brow[19] suggest that they ultimately overcame the agricultural peoples farther east, with their comparatively high-grade but conservative bronze culture. Evidence for a similar crisis has been found at Scarborough,[20] in a region which is, geographically, a continuation of the southeastern province.*

A few facts seem thus to be emerging out of the confusion that has covered the prehistory of Britain during the period (1100-500 B.C.) which coincides with that of Hallstatt in central Europe. That it was a time of change and progress on the continent is abundantly clear; and in Britain we have indications of wave after wave of invaders bringing with them new ideas and a new language with which to exchange those ideas. We have tried to show the distinction and importance of the first of those waves which swept across the continent and broke on the shores of Britain.

An interesting problem raised by this study of type-distributions is the partial interruption, during the late Bronze Age, of the ancient coastal routes of the west, the long survival of which is one of the fundamental facts in European prehistory. Though trade doubtless

* It is equally possible that the invaders landed in bands all along our eastern and southern coasts, as did the Saxons later, and probably also the Beaker people at the beginning of the Bronze Age.—ED.

played a subsidiary part in the diffusion of the industries examined in this paper, the old contacts between Brittany and Ireland seem to have been broken and the culture currents deflected along the English Channel.

Thus the far West came to be influenced more and more by the lowland province of England ; and we may see in this change the full realization of the culture focus of the western Alps, a region which synthetized the traditions of East and West, of the Danubian and western Mediterranean provinces, and introduced into our islands a scheme of life adapted to the environment of Europe north of the mountain belt, where it was evolved.*

Appendix

Inventories of hoards

I GUIDEL (KERHAR), MORBIHAN: Vannes Museum

A winged celt and fragments of others.
Portion of a socketed celt with vestigial wings.
Fragments of swords.
Fragments of socketed knives.
A chisel.
3 socketed spearheads.
3 razors.
5 pins (4 thistle-headed).
3 buttons.
Ornament stamped with concentric circle design.
Bronze leaf : fragments of twisted wire and hollow bracelets.
Rings, discs, and 'objets d'appliques '.
Jets and runners ; lumps of metal ; portion of mould.
' Nombreux débris méconnaisables '.

II AMIENS (le PLAINSEAU), SOMME : Amiens Museum (in part)

7 winged axes and fragments.
39 socketed axes and fragments.
32 fragments of swords.
27 socketed spearheads and fragments.
An anvil ; socketed hammer.
Sickles, gouges, knives, scrapers.
A portion of horse-bit (? belt-fastener).
A button ; a pin.
Bracelets, spirals, beads, rings.
Lumps of copper.
' Débris divers '.

* I have to thank Professor H. J. Fleure and Mr O. G. S. Crawford for suggestions and help of various kinds. I am also indebted to Dr Viollier for supplying the illustrations of Swiss swords and to Mr Louis Clarke for the photograph of the Cumberlow hoard from the Cambridge Museum.

III GRAYS THURROCK, ESSEX : see *Antiquaries Journal*, 1922, II, p. 105.

4 winged celts and fragments.
33 socketed celts and 71 fragments.
15 spearheads or fragments.
42 fragments of swords.
2 chapes.
Half of bronze mould ; 4 waste pieces.
A lump of tin ; 68 lumps of copper.
3 socketed knives and a tanged knife ; 9 fragments.
Tanged chisels.
A socketed gouge and 5 fragments.
A socketed hammer and fragments.
A fragment of sickle ; fragments of bracelets ; halberts.
A ferrule ; a ring ; 21 miscellaneous fragments.

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Excavation

by Commander Noel F. Wheeler, r.n. (retired)

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OUR only guide to the thoughts and motives of mankind is a correct interpretation of their acts ; and when we are dealing with the Past our only guide to their acts lies in a correct interpretation of the results of those acts. These results are visible in their writings, buildings, paintings, and in all the other works of their hands.

Time, with all its attendant destructive agents—war, wear, and wilful destruction—has obscured the evidence considerably; but much still remains, though it may require labour, patience, and careful deduction to secure it.

A man desires a home, builds a house—brick upon brick ; he lives in that house, and the presence of implements, utensils, and decorations is evidence of his use of them. The house becomes deserted; the elements throw down its walls, set up decay in the materials, until at last the débris of the years covers it. Then another man, finding a good site, rebuilds there after a different plan; and he shows his differing tastes in the objects he uses and in his methods of using them. The ordinary happenings of life go their round once more within the walls—working, eating, sleeping, birth, death, intrigue, crime, and all the heterogeneous patchwork which goes to make up human existence. An army overthrows the place, fire destroys its share ; but, after long ages, an archaeologist comes upon a mound among other mounds, and in the course of his operations he excavates this mound. The final result of his work should be a reconstruction, as complete as possible, of the past history of that house throughout its eventful or uneventful career, and of its occupants. The 'house' may be a cemetery, a fortress, or a tomb—it makes no difference to the main object.

The excavator's work may be clearly divided into six main parts,

(1) The discovery or choice of the site.

He may be influenced in this by knowledge handed down from remote ages, by obvious signs in the present appearance, or by the 'archaeological instinct' ; which is really the ability to sum up

accumulated probabilities and possibilities from a mass of scrappy impressions which would escape the lay observer.

It may be necessary to excavate a site merely on the strength of promising indications, without any absolute knowledge of what is likely to be found there. In this case subsequent events must be made to explain themselves. Where the nature of the site is known, all the attendant questions of supply of labour, living accommodation, attitude of the owners of the land or the authorities, food supply, water, etc., can be considered. The nature of the weather conditions likely to be encountered during the proposed period of excavation is a matter of some importance.

(2) The uncovering of the evidence.

In doing this the utmost care must be taken that no evidence, however trivial it may appear, be destroyed in the process of excavation. This process may take any of the forms intermediate between clearing a few square inches per day with a fine camelhair brush and lifting ten-ton boulders with a winch and tackle.

In the actual process of excavation it is necessary to employ more or less unskilled labour, and continual supervision is therefore essential. The employment of foremen should not take the place of this supervision, and the more direct the dealings of the director with the workmen the better : he is by far the most reliable ' foreman ' obtainable. The foreman, if left to his own devices, is apt to develop into a look-out for the men, to spur them on to greater efforts whenever the director appears on the horizon.

Where two or more levels of different ages are superimposed, and it becomes necessary to remove the upper in order to uncover the lower, the top level should be completely uncovered, photographed, drawn to scale, and levelled before a stone of it is destroyed. It is better that it should remain obscuring some older evidence below than that it should be partly or wholly destroyed unrecorded. A very complete photographic record is necessary, but it must not be allowed to take the place of plans and sections to scale, which should be accurate, clear, and adaptable to reproduction in print.

It is important to see that the area over which the débris is to be dumped has first been cleared and recorded. The money spent in Egypt alone in excavating the dump heaps of earlier excavators would endow more than one archaeological institute. No part of the area to be excavated should be neglected : the object of excavation is not to

search for one definite thing but to record faithfully everything within the area, whatever it may be and however unimportant it may seem at the time. Instances are numerous where the first excavators have, by incomplete work, missed the main significance of the site ; and the subsequent clearing of a neglected corner has revealed much. The true importance of an object found bears no relation to its size, cash-value, or beauty : the most uninteresting looking scrap may have more to tell than the rest of the season's digging.

In excavation time is always an object ; but, on the other hand, haste is the one thing to be avoided on first arriving on a site, with or without previous knowledge of the place and conditions.

Presuming that the question of living accommodation for the workers has been settled, a thorough inspection of the site is called for. As clear a mental impression of the place as possible should be formed from surface indications before putting pick to work. The general lie of the land will probably be visible in the case of fortresses, town sites, or even cemeteries ; and it is at this stage of affairs that aerial photography, if obtainable, may prove of the greatest assistance.

If a light railway is to be used for the removal of the débris, the direction and steepness of the gradient of the ground needs studying, and the most suitable area over which to dump should not be chosen without much forethought. A railway gradient should be steep enough to carry the cars with slight braking from the dig to the dump, and to allow of their being easily pushed back by hand. Sharp turns or sudden changes in gradient must be avoided for trouble-free working.

In any case the following considerations should be taken into account when dumping:—Dump over nothing that may need to be visible in the future ; reduce the travel of the cars to the minimum ; allow for the fact that the line of digging will be continually advancing ; avoid unsightly dumps on sites where appearance of the discoveries is likely to be of aesthetic value ; search the débris sufficiently thoroughly to preclude the possibility of any later excavators considering it worth while to search the dumps.

In forts or town sites the line of advance will clearly be along streets, if any, or parallel to lines of buildings ; in buildings, room by room. The walls make the best basis for survey points, owing to their height, mutual visibility, and relative permanence. In cemeteries the line of the work may have to be determined by cutting preliminary trenches to gain more information of the lie of the land than is visible on the surface. In well defined cemeteries an advance on a frontal line

is best to cover the entire area; but with widely scattered graves, invisible on the surface, this may not be practicable.

At first work will be necessarily slow, and *everything* uncovered should be left undisturbed until the director has inspected it. Later on, when the nature of the work is better known to all taking part in it, things can be speeded up and the diggers will know what to do with each thing uncovered. Records should be kept with the utmost care and completeness at the beginning until the significance of the discoveries is more clear to the workers, when they will get to know just what degree of detail and accuracy is demanded for the different kinds of evidence discovered.

The spirit of that expedition which sets forth in a blaze of self-advertisement and frantic enthusiasm to ' find something '—preferably something sensational—should be suppressed at birth. It is the spirit of the dog searching for a bone—earth, sand and everything flying in all directions in a blind concentration on the bone (which may not be there).

(3) The preservation of the evidence.

The objects or buildings uncovered may require treatment to preserve them before they can be dealt with. Weather, handling, packing, etc., must not be allowed to destroy what the ages have preserved intact hitherto. Expert chemical advice may be needed in this. If the objects can possibly be preserved they should be.

(4) The recording of the evidence.

A system must be used which ensures that the information recorded shall be easily available at short notice, and that it shall be well secured against destruction or confusion through lack of skill in the recorders, through transit, storing, or accident. This system must be simple but infallible—difficult requirements to satisfy in combination.

In recording nothing should be omitted. Every object found should be photographed, drawn to scale, numbered and described with reference to its material, colour, workmanship, place found, by whom and when. If there is any possibility of the object being in its original position of deposit, undisturbed, it should be photographed ' in situ '. It will be found useful to keep approximately the following records:—

(a) Rough diary of the work day by day, giving descriptions and rough plans of the area cleared daily, objects found, etc.

(b) Card index registers of the objects found ; one according to the location, and another according to the nature of the object. Each card should have a scale drawing and full description, date, where found, material, measurements, photo number, registration number (also on object), and notes as to other points of interest. The two indexes to be kept separately to minimize damage by fire, etc.

(c) Register of photographs taken, giving time, place, direction, etc. All photographs should have a measure included in the picture to give the scale.

(d) Photographic plates and a set of the prints should be kept in numerical order where they can be easily referred to at all times. To be kept separate from one another (the plates from the prints) to minimize risk.

(5) The interpretation of the evidence.

Here expert knowledge of the particular period, place, etc., is necessary ; and when this is not obtainable within the expedition itself it must be called in from outside. Theories without adequate evidence to support them are worse than valueless, and the former must rest on the latter rather than the reverse. It is very easy when searching for evidence to support a preconceived theory to get some strange and unstable results. It is wise not to be too precipitate in the formation of theories. By waiting until all the evidence is uncovered one is certain not to have the pet theories of one day overthrown by the excavations of the next. It is wise also to take into consideration everything that has ever been written on the subject in hand. One's ' probabilities ' may be cancelled by some fact unearthed previously and inadequately recorded in some obscure publication. Judgments can only be made on the sum total of the evidence, one's own and that of all others who have investigated the subject.

(6) The publication of the information found.

It is obvious that, so long as the information is retained in the hands and the memory of the excavator, the purpose of archaeology is but half served. Until the full information is in print the excavator cannot sit back with the feeling that a good piece of work is finished.

Ease of reference is one of the most necessary qualifications for a

publication; clarity of explanation and argument comes next in importance. The full data on which theories or deductions have been made should be given concisely, in order that the reader may form his own conclusions on the evidence—which conclusions are by no means bound to concur with those of the writer. These data should not fill page upon page with meaningless tabulation of numbers and letters, for the interpretation of which a continual reference back and forth in the book is necessary: the book is intended to be *read* and should not require to be fed into a calculating machine.

Plans should be easily readable: enlargement or reduction for printing will alter considerably the legibility of lines and figures, and this should be allowed for in the drawing.

N.B.—The above is merely intended as an outline, of the most general nature, of excavation in the broadest sense. According to the locality and the individual peculiarities of any particular work, so will the details need amplification, modification, or alteration, to suit the conditions. Experience in excavation in Egypt has formed the guide for this article, but the outline will be found applicable to most excavation work.

Prehistoric Flint Sickles

by E. Cecil Curwen

So much interest was aroused by my recent paper on 'Prehistoric Agriculture' in vol. I of Antiquity, that it may be worth while to expand and add to what was there said on the subject of flint sickles. The most comprehensive study of the subject comes from the pen of M. André Vayson de Pradenne[1] whose paper must first be reviewed.

The author sets out by describing an almost perfect example of a flint sickle, consisting of five carefully worked flakes set in an L-shaped wooden mount, discovered some years ago in a peat-bog at the foot of the hill of Solferino, near the Lago di Garda in North Italy (fig. 1). After describing its characteristics and discussing its purpose, he reviews all the other known specimens from Europe and Africa, and finally discusses their types and distribution.

The Solferino specimen is attributed to the Copper Age, having been found at the same level as a copper palstave and some flint arrowheads, and below the level of an early bronze dagger. In fact the only remains found in those bogs belong to that period, the full Bronze Age being unrepresented.

The sickle consists of an L-shaped piece of wood with a longitudinal groove in which a row of five flints have been cemented. The tip of the handle is missing, and the body was broken in two while being carelessly dried by the finder, but accurate reconstruction has been possible. The wood has been cut from the fork of a tree so that the grain runs longitudinally both in the blade and in the handle. The latter is slightly inclined to the plane of the blade, as is that of a modern scythe, and the author regards this as a sign that the implement was intended for cutting stalks close to the ground, and therefore possibly for mowing hay rather than for cutting corn.

Each flint has been worked to fit in with its neighbours, the ends being bevelled to fit a corresponding bevel on the next flint. The cutting

[1] 'Faucille préhistorique de Solférino', *L'Anthropologie* (1919) XXIX, 393–422.

edges of the five ' teeth ' form a perfectly continuous curve. There is no polish on them, but they are patinated white. The ' teeth ' are of three types, viz., those for each of the two ends of the row, and the intermediate ones. These three types are distinguishable among the sickle-flints found out of their setting in the Italian lake-dwellings and in Egypt.

The flints project not more than 12 mm. out of the groove in which they have been cemented with a brown resinous cement. The walls on either side of the groove are 2–4 mm. thick.

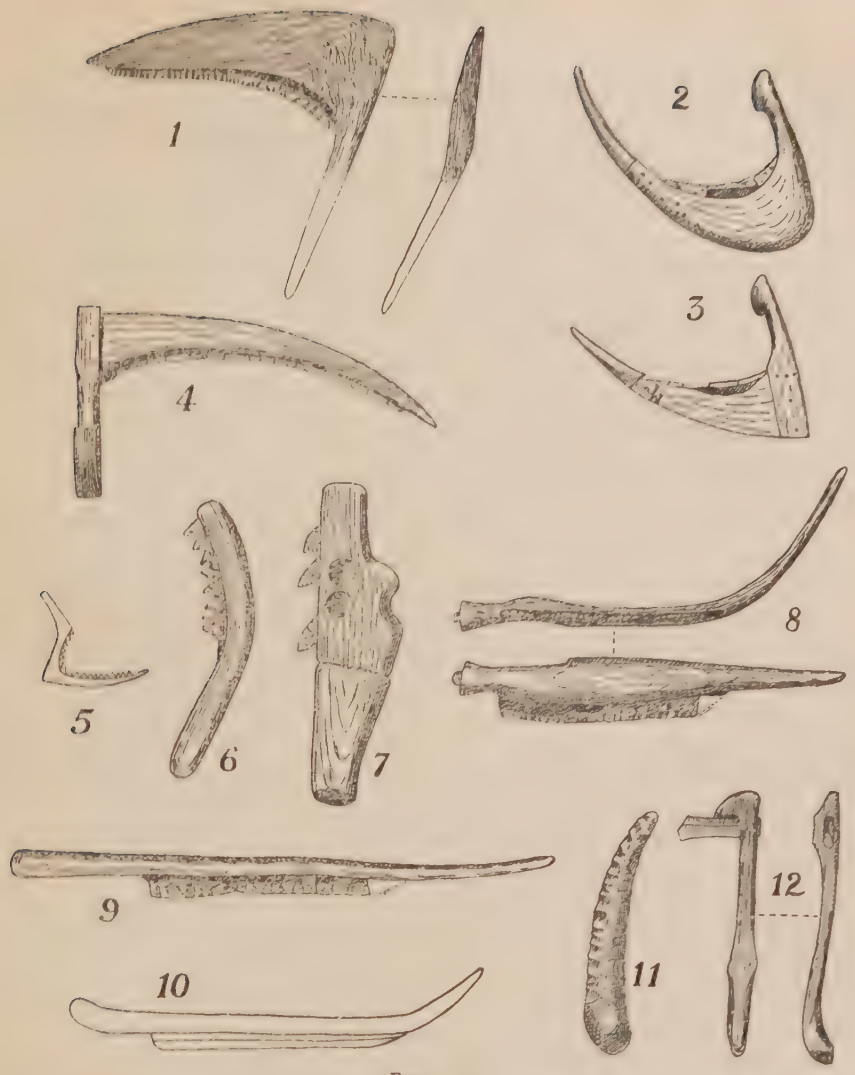
At Polada was found another specimen which differs from the last chiefly in the fact that the handle lies in one straight line with the blade when seen in profile, but is bent to an angle of about 135° from the plane in which the teeth lie (fig. 8). This latter gives it the appearance of having served as a scythe rather than as a sickle, just as in the Solferino example. In respect of their dimensions, and the shape and mode of insertion of the flints, these two sickles are similar.

Polada has also yielded another similar specimen from which the teeth have been lost, and fragments of a third, much warped and decayed.

Flint teeth, precisely similar to those described above, are numerous in the Italian lake-villages, and have usually been described as saws. Others are similar but larger, with ends not trimmed to fit in with their neighbours, and these have probably been mounted singly as knives. Flakes worked on both faces are peculiar to the lake-dwellings ; those worked on one face only are common throughout Italy, and are regarded by the author as an older and more primitive type. Each type has probably been used both for ' knife-saws ' and sickles. The author suggests that if the ends have not been trimmed to articulate with other flints, they must probably not be regarded as sickle-flints. This, however, seems to be too sweeping a judgment, for the highly developed sickles of Solferino and Polada must have had an evolution from more primitive beginnings.

With regard to the question of polish on the cutting edge of these flints, M. André Vayson is at variance with Mr Spurrell,[2] for he says that polish is neither necessary nor sufficient as a criterion of use. The Solferino specimen had no lustre because the white patina has, he considers, destroyed it. Moreover he has carried out experiments which show that, contrary to Mr Spurrell's findings, the sawing of wood polishes flint just as much as the cutting of corn or grass.

[2] *Arch. Journ.* XLIX, 53.



Figs. 1–12

References to Figures

1. Solferino (restored: after Vayson)
2. Kahun, Egypt, 12th dynasty (Sir Flinders Petrie)
3. Kahun, 17th dynasty (Petrie)
4. Acebuchal, Spain (after Vayson)
5. Triangular-toothed sickle, from inscription at Meidum, Egypt, 3rd dynasty (after de Morgan)
6. Cueva de los Murcielagos, Spain (after Gougora)
7. Fenil, Lac de Bienne (after Vayson)
8. Polada (after Munro)
9. Fayum, Egypt (Miss G. Caton-Thompson)
10. Straight sickle engraved on dolmen of Ile-Longue, Brittany (after Vayson)
11. Yarmouth (after Evans)
12. Stenild, Jutland (after Déchelette)

A different kind of sickle has also been found in the Swiss lakes at Fenil (lac de Bienne), consisting of a straight handle with more or less triangular flint teeth set in a groove, and each projecting independently—quite an effective instrument for cutting corn (fig. 7).

The author then describes Sir Flinders Petrie's two Egyptian sickles from Kahun (12th and 17th dynasties), which closely resemble the Solferino specimen in most particulars (figs. 2 and 3). Sickle-flints, he says, are numerous in the sites of the early dynasties, but rare or absent on purely neolithic sites. The serrations may be coarse, fine or irregular, and the working may be on one or both faces. Polish is common, and traces of cement are frequently seen.

Triangular flints are also found, analogous to those of the Fenil sickle. A sickle bearing triangular teeth is actually depicted on a third dynasty inscription at Meidum (fig. 5).

Similar sickle-flints are abundant in Chaldea and Palestine where, according to Sir Flinders Petrie, they continued in use till 1350 B.C. or even later ; and they are also found in North Africa and Morocco.

M. Vayson quotes M. Cartailhac as stating that such flints are found along the northern shore of the Mediterranean from Spain to Asia. Numbers, he says, have been found in the neighbourhood of Almeria (Spain), in the ruins of ancient fortresses, where they are exclusively found in the dwellings of grain-merchants and millers—dwellings distinguished by numerous querns and jars containing barley and flour. With these were found small collections of these sickle-flints. Similar specimens come from Therasia and Santorin, also from the whole of Greece, and from most of the ancient levels at Troy.

From Acebuchal in Spain (Seville) comes a reconstructed wooden sickle of the Solferino type, with numerous small rectangular teeth with coarse serrations, and with ends not so carefully shaped as in the Italian and Egyptian examples (fig. 4). The frame is made of two pieces of wood, a blade and a handle, fastened together at right angles. This specimen is referred to the Copper Age.

From Cueva de los Murcielagos (Spain) comes another specimen, very slightly curved, and having eight triangular teeth (fig. 6).

In France are found neolithic flakes with polished serrated edges, but the ends are not trimmed to fit one another, and they are very rare. They are not flat, but curved, and therefore the author considers that they cannot have been used in sickles. Much more abundant in late neolithic sites are triangular flints, of which some, being sharply pointed and isosceles, might be arrowheads, but others have no point and are

asymmetrical (scalene), resembling the teeth of the Fenil and Murcielagos sickles.

In Britain both the Solferino type of sickle-flint and the triangular type are found, and in addition to these the long curved flint-blades similar to the Scandinavian type described below.

In Scandinavian countries the rectangular and triangular types are rare or absent. Here the characteristic sickle was a long, curved blade, finely worked all over, and often showing polish on the edge (fig. 11). Their method of hafting is probably illustrated by the discovery at Stenild (Jutland) of an unworked flake fixed at right angles into the end of a straight wooden handle (fig. 12). The shape of the Scandinavian flint sickle is identical with that of the earliest bronze sickles of Central Europe.

M. André Vayson summarizes his conclusions to the following effect :

(1) Flint sickles were used at any rate from the Copper Age to the Mycenaean period (2000–1000 B.C.).

(2) Two types of frame were in use, the straight, and the L-shaped, the latter resembling the lower jaw-bone of an animal. In fact, according to Maspero, the ancient Egyptian word for ' jaw-bone ' signifies a ' pair of sickles '.[3]

(3) The sickle-flints were of 3 main types :—(a) rectangular ; (b) triangular ; (c) long, curved blades. The first two are common to the Swiss and Italian lakes, Egypt, Spain, Central Europe and Britain. The third is found chiefly in Scandinavia, but also in Britain and Central Europe, and is analogous in form and size to the earliest bronze sickles.

Probably the simple serrated flakes and the triangular teeth are more primitive than the well-worked rectangular flints or the large curved blades.

In another communication[4] M. Vayson draws attention to what is evidently the representation of a straight-handled flint sickle on the dolmen of Ile-Longue in Brittany (fig. 10), and compares it with a remarkably fine example (fig. 9) recently discovered by Miss G. Caton-Thompson in the north of the Fayum, and figured in ANTIQUITY, 1, 336. The similarity is striking and convincing.

So valuable a paper as M. Vayson's is worth recording in English,

[3] Déchelette, *Age du Bronze*, 267.

[4] *Bull. de la Soc. Préhist. française*, XXV, 290–4.

even if only in the form of an abstract, and there is little to add to it except to discuss the vexed but important question of the polish accruing to the flints as a result of use.

Many, but not all, of the sickle-flints found in Egypt, Palestine and Mesopotamia have a wide band of lustre extending along both sides of the serrated edge to a depth of a quarter of an inch or more (see plate, no. 2); in fact the depth of the band has only been limited by the cement which once fixed the flint in its wooden frame. These sickle-blades are commonly made of chert and usually have coarse serrations.

Quite a different kind of lustre occurs on many of the finely serrated flakes which are found in large numbers on the sites of English neolithic camps, as, for instance, at Windmill Hill (Avebury), the Trundle (Goodwood), and Whitehawk (Brighton). These flakes are generally patinated white and have about 25 to 30 serrations to the inch, each notch having been pressed out from one and the same face of the flake. The lustre, which is to be seen on less than half the specimens, consists of a very narrow band of bright polish which is confined to the actual serrations on the plain face (see plate, no. 1).

Quite clearly these two kinds of lustre have not been produced by the same agency, and the determination of this point should go a long way towards solving the problem of the use of serrated flakes found out of their original setting.

Experiments carried out in the past with a view to solving this problem have not entirely succeeded. Mr Spurrell[5] found that corn was the only substance the cutting of which produced a polish on the flint; M. Vayson, as noted above, found that both corn and wood were capable of producing this effect, but he records no effort at differentiating them. In order to investigate the matter afresh the present writer obtained a series of newly made serrated flakes of black flint from Fred Snare of Brandon, the serrations being coarse in some and fine in others. Separate flakes were used for cutting wood, dry bones, and corn-stalks in the form of bottle-straws.

(1) Wood.—It was impossible to cut into the wood to a greater depth than about $\frac{1}{8}$ inch owing to the inevitable v-section of the flake. With this limitation flakes with fine serrations bit into the wood as readily as a sharp steel saw. After spending three-quarters of an hour in making a series of nicks in sticks and oak logs the degree of lustre shown in photograph no. 3 (see plate) was attained. This is quite

[5] *Arch. Journ.* XLIX, 53.

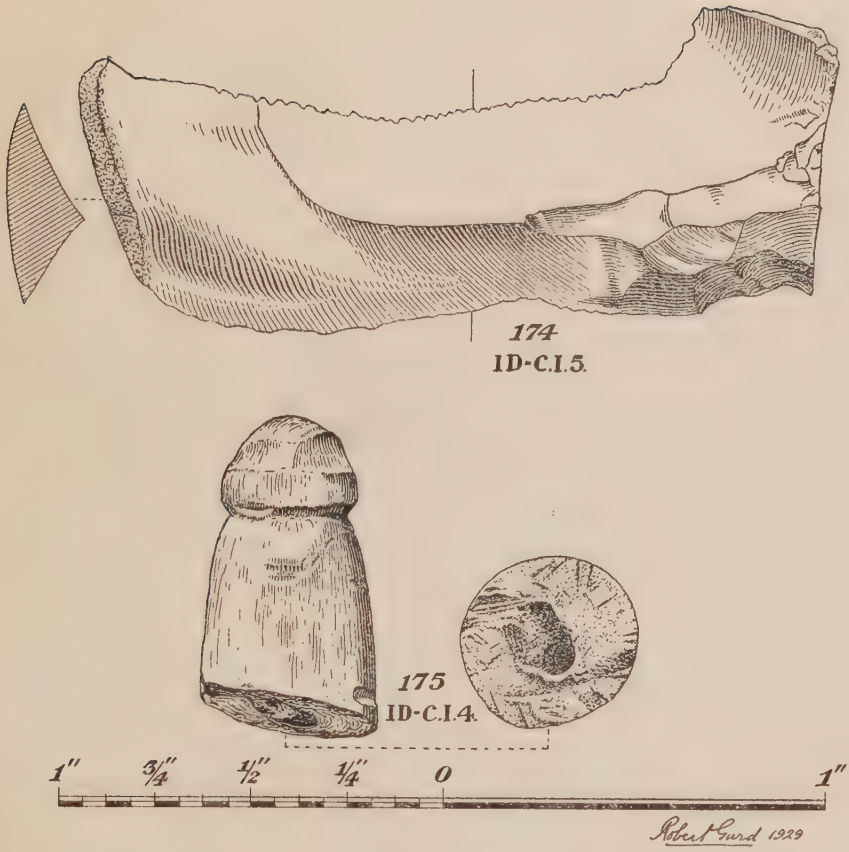


Fig. 13

Flint saw, and a neolithic carved bone object that has been severed by means of a flint saw (from the Trundle, Goodwood)

(After *Sussex Arch. Coll.* lxx)

comparable to the polish in no. 1, but extends rather further from the edge, probably because this flake was used during part of the time for making oblique or sloping cuts.

(2) BONE.—Prolonged efforts failed to produce an appreciable degree of lustre when sawing old dry bones.

(3) STRAW.—Half an hour spent cutting bottle-straws up into little bits produced the degree of lustre shown in photograph no. 4 (see plate). It is a broad and diffuse band like that in no. 2, and it was evident that the greater brilliancy of polish in the latter case must have been the result of a great deal of prolonged hard work in reaping corn.

It seems fairly certain, therefore, that the narrow band of lustre seen in no. 1 results from nicking wood, while the broad band in no. 2 comes from cutting corn. In other words, the one is a saw and the other is a sickle-flint. The fact that polish is produced by both substances is due to the presence of organic silica in both wood and straw.

The nicking of wood by a serrated flake was doubtless a preliminary to breaking it. If such a tool was used on wood it would certainly be used on bone as well. During excavations at the Trundle the writer found in the neolithic level a beautifully carved little bone object, which must have been shaped with flint tools (fig. 13). It had been severed from the end of a bone by sawing all round to a depth of about $\frac{1}{8}$ inch and then snapping it off—a striking testimony to the use of flint saws on bone.

The writer has not so far been able to trace any serrated flakes from Britain bearing the kind of gloss that is produced by cutting corn. M. Vayson thinks that the patination which many of the flints have undergone has destroyed any lustre that may have once existed. This is open to question; at any rate the extremely brilliant lustre caused by the friction of wood has not been impaired by the patination of the flints on which it occurs. A degree of lustre likely to survive is not, however, easily produced by cutting straw; moreover our flint may not polish so easily as the chert of Palestine and Egypt, so that one may be fairly safe in assuming that lustre is not essential as evidence that a given flint formed part of a sickle, especially as it does not occur on many undoubted sickle-flints found abroad in their original wooden frames.

These observations are made, not so much as expressing settled opinions, as with a view to elicit observations from those who have the handling of large numbers of flint implements, in the hope that more definite information may thereby be obtained.

Butser Hill

by Stuart Piggott

Investigator, Royal Commission on Ancient Monuments in Wales and Monmouthshire

THE traveller from London to Portsmouth by road, as he leaves Petersfield (fifty miles from his starting point and twenty from his objective) sees before him, above the copses and hopfields, a great green hill like the overturned hull of some gigantic ship. Looking southwards, he sees to the east the line of the Sussex Downs and to the west the less definite Hampshire ridge, and between them this majestic hill—Butser. Approaching nearer, the spurs which run out from the main mass show clearly the sunken tracks that wind up to the high level plateau above ; and through the deep road-cutting across the col connecting Butser with Wardown on the east, a green land of ridges and hollows, of downs studded with juniper and thorn and of coombes with their sides covered with yew and whitebeam is entered. A yard from the road and there is untouched downland where one may walk all day and see no one but the occasional desecrating workman who strips Butser of its turf so that the suburbs of Southsea may have tennis courts.

Now should our traveller have the good fortune not only to see Butser from the ground, but from the air, he would then see how the line of the Hampshire chalk ridge, running eastwards from Old Winchester Hill, here turns north, and as it turns, becomes Butser. From the valley Butser may resemble a vast overturned boat ; from above it is a hand, joined by an arm of narrow high ground to the main east-west line of the ridge. The back of the hand is the nearly level top of Butser, gently sloping away from the central height of 889 feet (the highest point of these downs), and the fingers and thumb are the projecting spurs separated by deep coombes. To the east, Wardown rises to a height of 802 feet, and from there the ridge swings gently southeast to resume the general line in the Harting Downs.

Such a hill as Butser would obviously have attracted the attention of primitive man as a desirable place for residence, cultivation or burial ; and we might expect to find evidence of one or more of these.

We shall see that there are traces of use in all these ways, and in more than one period.

The general plan (fig. 1) of the hill shows that the antiquities of Butser divide themselves into five main groups: the trackways, the entrenchments across the spurs, the lynchets, the bivallate ditches, and the barrows and other mounds. While these groups are occasionally inter-linked, and explain each other a little, it will be convenient to study them first in detail under the above headings.

The Trackways

Butser occupies a commanding position among the hills of the South Downs, and this is well brought out by the numerous tracks which converge on and radiate from it.

First in importance is the track which links the almost detached massif of the hill itself with the downs to the south. This roadway, which runs along the ridge of Hillhampton Down, is part of the great ridgeway which can be followed west to Winchester and which on the eastwards runs along the whole length of the Sussex Downs. Absolute dating is of course impossible; but there can be no doubt that this ridgeway is of great antiquity.

When the track reaches the neck of Butser near the main entrenchment on the south spur, it sends off several branches; but the main track continues east along the edge of the plateau until, on the spur immediately over the modern road-cutting, it again becomes involved in a jumble of sunken tracks, some of them mere duplications, others contributory ways. On the western slopes of Wardown, across the col, the track ascends by a slight diagonal terrace to the top, and is there continued (with breaks) to Harting and beyond.

These sunken tracks on the eastern spur are a striking feature as seen from the modern high-road; they consist of roughly parallel deep v-shaped trenches curving up over the shoulder of the hill, and to those unacquainted with earthworks they might well appear to have been deliberately cut (plates I and II). They are however the unintentional result of traffic negotiating a steep slope.

The history of such sunken tracks is roughly as follows. A single trackway is originally started on the hillside. The turf is soon broken up by the hoofs of horses or the surface grooved by cartwheels, and once the protecting mantle of turf is removed, the chalk rapidly wears away; the frosts break it up and the rain soon converts the trackway into a slippery gulley of chalk mud. Perhaps this mud is cleared out

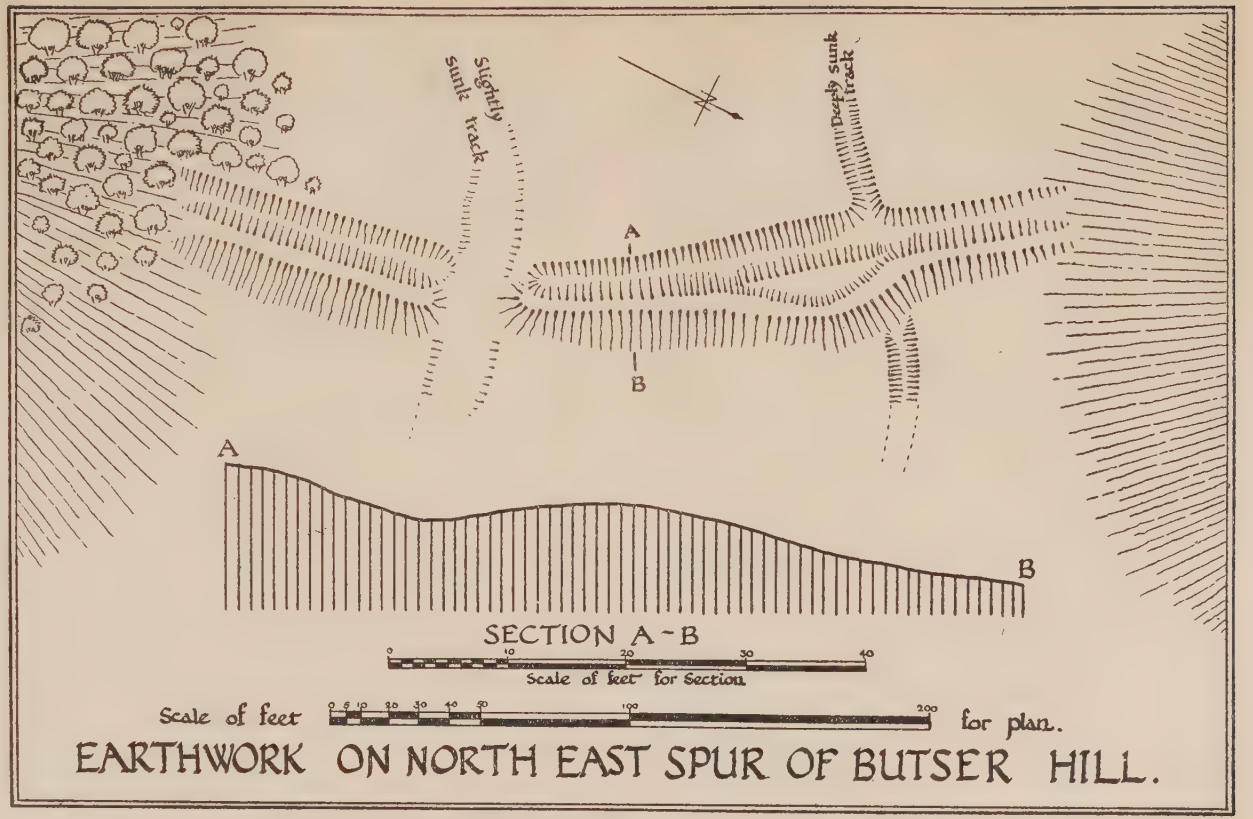

Slightly sunk track
Deeply sunk track
A
B
A
B
SECTION A-B
0 10 20 30 40
Scale of feet for Section
Scale of feet 0 5 10 20 30 40 100 200 for plan.
EARTHWORK ON NORTH EAST SPUR OF BUTSER HILL.

FIG. 2

and thrown out on one side (making a deceptive bank to puzzle the unwary field archaeologist). Another winter and the track is sunk still deeper, until at last it becomes impracticable. Then a parallel course, probably further downhill, is followed. This has a history like the first and so yet another is formed. The older tracks gradually become grass-grown again and may perhaps be re-used, but the inevitable result is a series of roughly parallel trenches scoring the hillside.

Sunken tracks in groups such as these on Butser are quite common on the Downs, and while some may be very ancient, many (especially the broader ones resulting from wheeled traffic) are comparatively modern. A relative date is given to the Butser examples by the fact that they cut through a large lynchet-bank running parallel to the modern main-road, thus showing that they are later than that bank. We do not know the exact date of the large group of ancient fields of which this bank forms part; but they are certainly not later than the Roman period and are probably earlier. It is likely, as Dr Williams-Freeman has suggested, that these sunken tracks are due in great part to the carting of flints from the hill-top (still scarred with irregular shallow pits) when the modern road was being built some two generations ago.

That the ridgeway between these tracks and the south spur entrenchment was an important road at least until the 17th century is shown by the bounds of the manor of East Meon taken in 1647 (printed in *Procs. Hampshire Field Club*, IX, 413). The bounds are running, as usual, with the sun, and proceed from the Buriton direction; 'and so abutting upon the Manor of Berriton and Maple Durham upon the southeast unto a great ash standing on the side of Butser Hill, and so by the highway leading to two great ditches cast up at the top of Butser Hill'. The 'ditches' in question can only be the main entrenchment and ditch II (see main plan of Butser).

Trackways run up the three northerly spurs to join the main ridgeway; there being one up the unnamed spur south of Rake Bottom, another up Ramsdean Down, and a third up the spur (above Round Copse), which has been called the Northeast Spur. This last trackway is the most important of the three and in all probability the most ancient. From near Round Copse, on the rounded knoll locally called Little Butser, a deeply sunk track winds up the nose of the spur, to die out on the level. Before reaching the plateau it is joined by a fainter track from the southeast. Just above this junction there runs from scarp to scarp the curious earthwork named the Northeast Spur entrenchment. This will be fully

EARTHWORKS on BUTSER HILL, PETERSFIELD, HANTS.

FIG. 1

facing p. 190

described in its place; here we are only concerned with its relation to the sunken track and its branch. The track is cut through by the ditch of the entrenchment and obliterated by its bank; the branch cuts through the bank and ditch. Clearly the main sunken track is older than the entrenchment, the branch contemporary or later.

The date of the entrenchment is not known, but the type to which it belongs has early associations and can hardly be later than the Early Iron Age at the latest. The track must already have been in existence for some time before the construction of the entrenchment, to allow for its depth, and it must therefore be regarded as an indubitably ancient roadway leading into the valley.

On the spur the track points northwards towards Petersfield, and its line is continued from the foot of Little Butser by the lane running north to Bopeep Copse; from there it may have turned eastwards through the hamlet of Weston. This course keeps above the 300-foot contour line, but between Weston and Petersfield there is low-lying land to be crossed. A lane from Weston running northeastwards is a deep hollow-way where it drops down the slope near the 300-foot contour, and this hollowing can only be the result of traffic far more continuous than the present almost disused footpath warrants. North of this point the track may have joined the present Portsmouth road near the Causeway, or followed the footpath across the fields from Weston to the western edge of Petersfield at the Borough.

An ancient road leading directly into the low-lying and somewhat marshy land round Petersfield might appear at variance with the usual concentration of prehistoric remains on the high dry uplands; but there is the very interesting and indisputable fact that on Petersfield Heath is a large group of round barrows, disc and bowl, and it is unlikely that the settlement to which these belonged was situated three miles away on the chalk ridge. Butser, too, has its round barrows, and this track may well be as old as the Bronze Age, joining the two settlements.

The Spur Entrenchments

Between the spurs of the Butser plateau there are deep narrow coombes running back into the hill, with steep and sometimes almost precipitous sides. The spurs provide the only easy access to the hill-top from the valley, and any attempt at fortifying the plateau, short of constructing a complete hill-fort on the top, would naturally lead to the construction of banks and ditches across the spurs from scarp to scarp, thereby cutting off these ways of approach.

Something of this kind seems to have been attempted, but was carried out incompletely and in unusual ways. The most vulnerable point is where Butser is joined to Hillhampton Down by a narrow level neck, between two very steep coombes. Across this neck is constructed the main, or south spur entrenchment. (This is described fully below). On the spur to the east of this earthwork (the southeast spur) a slight irregular bank and ditch have been constructed across the root of the spur, in two sections with a gap of thirty yards between them. The bank is about 3 ft. 6 in. above the bottom of the ditch and the overall measurement is some 30 to 40 feet. The whole earthwork is mutilated by flint digging.

Only two of the other five spurs have entrenchments across them : the spur above Round Copse (the northeast spur) and that south of Rake Bottom (the southwest spur).

The northeast spur has been described above, but not the earthwork upon it. The most striking feature of the entrenchment (fig. 2), which cuts across its root, is that its ditch is on the *up*-hill (south) side; and, if it is to be regarded as a part of the defences of Butser, it must be supposed that the bank faced the attacking force. But from what we know of the principles of prehistoric fortification we may say that it was an axiom always to be above one's enemy—on a bank with a ditch facing towards him. It is difficult then to regard this earthwork as defensive. Another curious feature of its construction is that at one point the bank swings out from the ditch in a crescent, leaving a semi-circular berm of undisturbed ground between its crest and the lip of the ditch.[1] It has already been noted that this entrenchment cuts across a pre-existing track which comes up the spur.

The southwest spur of Butser is traversed by two precisely similar earthworks, with an interval of some 100 yards between them. Their profile is the same as that of the entrenchment on the northeast spur —the ditch averaging 3 ft. 6 ins. below the bank and the overall width being some 50 ft. Here again the ditch is on the up-hill side. The lower of the two entrenchments appears to be discontinuous or mutilated, and on the higher one there are remains of an upper bank. What appears to be a lynchet bank joins this at right angles on its south end.

The purpose of this type of entrenchment is very obscure. Dr Cecil Curwen, in describing similar earthworks on Bow Hill, near

[1] This peculiarity is paralleled in a similar earthwork in Wilts., near Burcombe Punch Bowl. See Heywood Sumner, *Earthworks of Cranborne Chase*, plan xxxv, and p. 63.

Chichester,[2] draws attention to their association, there and on Butser, with Bronze Age barrows, bivallate ditches of the 'covered way' type, and a water-hole. Somewhat similar associations may be cited from Wiltshire: two such earthworks near Burcombe Punch Bowl with a round barrow near,[3] and one on Buxbury Hill near the bivallate ditch known as 'Row ditch'.[4]

Another possible explanation is suggested by the presence of two such ditches on spurs near the Trundle—a refortified Neolithic camp; while the flint-mines on Bow Hill and the long barrows on Stoughton Down nearby should be noted. As we shall see, there is some reason to suppose that on Butser there is a refortified Neolithic earthwork.

All these associations, of course, *prove* nothing, and give no clue at all to the use of the earthworks in question. The writer's suggestion is that they belong to the same period (or periods) as the bivallate ditches, and that their use was the same as the presumed use of these.

The Main Entrenchment

The southeastern spur of Butser Hill unites the main mass of the hill to the level ground of Hillhampton Down by means of a narrow neck, and it is across this neck, at the Butser end, that the main entrenchment of the hill was constructed. In plan it is markedly convex towards Butser and presents a concave face on the southern (ditch) side. As Dr Williams-Freeman has pointed out,[5] this apparent weakness is really a sound adaptation of the plan to the lie of the land. On the ground the concavity of the plan is scarcely apparent.

The entrenchment (fig. 3 and pl. III) consists of a single bank and ditch running in a general NW–SE direction between the two 700-foot contours. Both bank and ditch are very irregular and are broken through at three points by trackways: one of these, the most easterly, represents the main ridgeway of the South Downs. The greater part of the entrenchment has its ditch separated from its bank by a wide berm, some 16 feet wide at its widest. The ditch, 30 feet across on an average, is most irregular, and has been deepened at frequent intervals into unequal troughs and hollows, from 4 to 5 feet deep. A shallow ditch runs continuous with the bank except at the three

[2] *Prehistoric Sussex*, 1929, pp. 140–1.
[3] Heywood Sumner, *op. cit.*
[4] Heywood Sumner, plan XXXVI.
[5] *Field Archaeology of Hampshire*, p. 273.

breaches mentioned. Where the ditch is in its original state, forming apparent causeways between these deepenings, there are corresponding dips in the crest of the rampart. The impression given by a study of the earthwork on the ground is that it is a construction of two periods. In its original state it appears to have consisted of a bank and ditch running from scarp to scarp in four straight stretches, each change of direction being marked by a break in the ditch and probably also in the rampart. At some subsequent period, when the rampart had become denuded and the ditch silted up to a considerable extent, an attempt was made to refortify the site by digging out the silting from the ditch and piling it up on the old rampart. The present height of this rampart above the bottom of the ditch is about 13 feet, and it is 5 feet above the ground level to the north. Perhaps men were employed in gangs to dig out sections with a view to clearing away the 'causeways' afterwards, but everything points to hurried and unfinished work. The irregular excavations, the consequently uneven rampart, and the berm all indicate a hasty temporary strengthening of an old entrenchment.

The question naturally arises as to the actual dates of these two periods of construction. Without excavation we cannot be certain, but a tentative suggestion may be advanced, supported by such data as are available.

The earliest period of earthwork construction in England appears to have been towards the close of the New Stone Age, when entrenchments of the 'interrupted ditch' or causeway type were constructed. They consisted of one or more enclosing banks and ditches, the latter broken at frequent intervals by causeways of untouched soil. The type example of this construction is the famous site at Windmill Hill, Avebury, which is being excavated by Mr Alexander Keiller.

'Causewayed' earthworks which take advantage of natural defensive features have already been described by Dr Curwen in ANTIQUITY, IV, p. 22 ff. The camp on Knap Hill, Wilts, partakes of the nature of a promontory fort.[6] A similar earthwork, though unproved by excavation, is recorded from Dinas, Llanidloes without, Montgomeryshire.[7] A modified Neolithic earthwork of this type was discovered on low-lying land near Abingdon, Berks., running across

[6] *Wilts. Arch. Mag.*, XXXVII, 42-65.

[7] *Sussex Arch. Coll.*, LXX, 72.

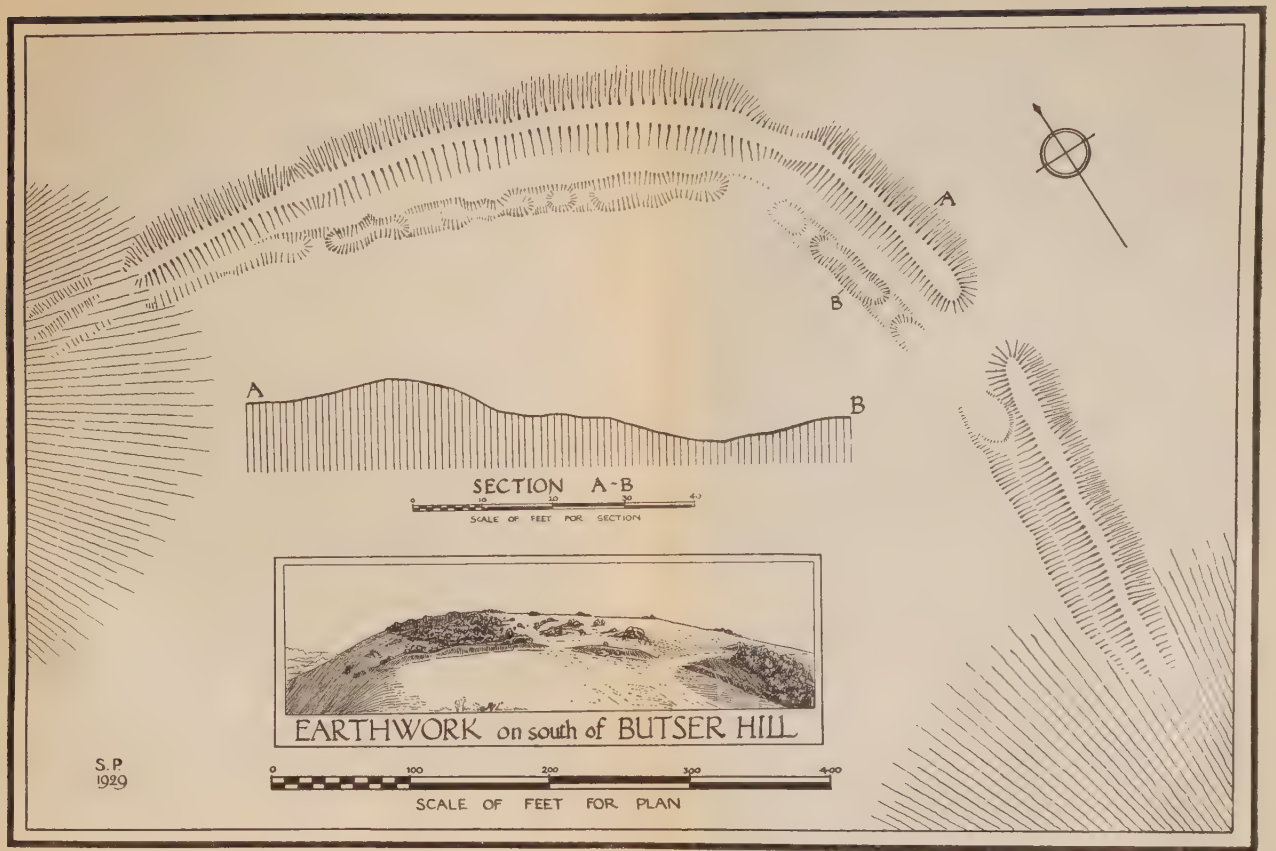


Fig. 3

facing p. 104

land between two convergent streams.[8] The 'Small Camp' on Hambledon Hill has its outlying spurs protected by double scarp-to-scarp ramparts which are, it is true, broken at frequent intervals, but Dr Gardner[9] is inclined to think that this discontinuity is caused by flint digging and carting. Whatever the date of the small camp may be, it is, as he has demonstrated, clearly earlier than the Great Camp (i.e. before the end of the first century B.C.); the later camp incorporating one of the outlying ramparts of the earlier in its fortifications. This adaptation of an earlier earthwork may perhaps be paralleled at Butser.

It should be borne in mind, that while we know 'causewayed' earthworks to be Neolithic, it does not necessarily follow that *all* Neolithic earthworks were of this peculiar type; and of Bronze Age earthworks we are as ignorant as we were of Neolithic earthworks twenty years ago. In this absence at present of any evidence for Bronze Age earthworks (other than barrows) there remain only the Early Iron Age and the Roman occupation, to one or other of which we may assign the refortification of the Butser entrenchment. The work has none of the precision and care characteristic of Roman earthwork construction, and if, as may be possible, it was altered in Roman times, it would have been late in that period. We are left then to consider the possibilities of an earlier earthwork being reconstructed in what might be called 'the hill-fort period'.

In the north of Hampshire there is an earthwork which admits of no other explanation of its construction, and which in many respects resembles the Butser earthwork. This is Ladle Hill camp, on a hill south of Sidmonton. The ditch is deep, interrupted by twelve causeways, and encloses an oval area of about seven acres. The rampart is irregular, dipping opposite the causeways, and in places there is a berm between it and the inner edge of the ditch.

There are other corroborative details which need not occupy us here, but it is almost certain that in Ladle Hill we have a Neolithic 'causewayed' entrenchment partially refortified in Early Iron Age times, but never finished.[10] It seems probable that the attempt was

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[8] *Antiq. Journ.*, VII, 438-464; VIII, 466-77.

[9] Dr Eric Gardner, 'Hambledon Hill' in *Wessex from the Air*, pp. 44-7.

[10] This suggestion, first put forward at a meeting there of the Hampshire Field Club (August 1929), is strikingly confirmed by a fine series of air-photos taken last winter. We hope to publish a detailed description of Ladle Hill in a forthcoming number.—ED.

given up and a fresh start made on the opposite hill, Beacon Hill (Burghclere), which is crowned by a fine and typical Iron Age hill-fort.

This reconstruction of a Neolithic entrenchment in Early Iron Age times is not unique: the conspicuous hill-fort on the Trundle, Sussex, appears to have been constructed on the outer ditch of a Neolithic camp[11]—in this case the work was completed and the causeways cut away—and probably the same thing has happened at Scratchbury and at Yarnbury Castle, Wilts.

Has the main entrenchment on Butser Hill a similar history? At Butser we have three distinct causeways between the lengths of ditch: the possibility of others having been cut away at the reconstruction must be considered. Each section of ditch is more or less straight, and when a change in direction is required the ditch stops, a causeway is left and another ditch started at a different angle. This 'inability to turn a corner' is a noticeable feature of Neolithic camps: when seen on an excavated site (as at Windmill Hill) it is very striking. So far as can be seen, the Butser ditch appears originally to have been wide in proportion to its depth and it may have been flat-bottomed. This latter feature is characteristic of Neolithic ditches, which contrast in this respect with v-shaped ditches of Early Iron Age earthworks.

Summing up the evidence, such as it is, the provisional theory is advanced that the main entrenchment of Butser Hill was originally constructed as a 'causewayed' earthwork in the latter part of the New Stone Age and hurriedly reconstructed in Early Iron Age times. Of its intervening history we know nothing; the Bronze Age barrows on the hill to the north are silent witnesses pointing to the occupation of Butser in that period. They are all that remains to record an interval of time nearly as great as that which separates the reconstructed earthwork, (now grass-grown and deserted by all save the rabbits and archaeologists), from the motors humming on the main road in the valley far below.[12]

On the south side of the earthwork are traces of depressions dotted over the ground. These may be remains of hut-sites, but the whole hill is pitted with modern flint digging.

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[11] *Sussex Arch. Coll.* LXX, 36, and ANTIQUITY, IV, 32.

[12] It may be worth while noting that there is a fine disc barrow immediately outside Ladle Hill camp, and another near Coombe Hill (neolithic) camp, Eastbourne; and there are groups of round barrows within the earthworks at Windmill Hill and Scratchbury, Wilts.

BUTSER HILL

The Lynchets

On the southeast slopes of Butser is an extensive group of lynchets (fig. 1 and pl. 1) of the 'chess-board' type, consisting of those approximately rectangular terraces which result from ploughing small fields on sloping ground. Similar lynchets occur over the greater part of the untouched downland to the east, south and west of Butser, and they call for no special comment. The researches of Mr O. G. S. Crawford and of Doctors Eliot and Cecil Curwen have established the main facts about these ancient field-systems, which they have shown to date at least from the Late Bronze Age or earliest phase of the Iron Age up to the end of the Roman Occupation.

It is difficult at present to form an opinion on the age of such a field-system from its general plan. Dr Cecil Curwen however, has suggested that Romano-British lynchets are characterized by a tendency for the individual fields to be square and not arranged in definite lines or strips, and he cites the group on Kithurst Hill, Sussex. By this criterion the Butser group would appear to be of pre-Roman origin. From the rabbit-scrapes and mole-hills over the area of the lynchets numerous fragments of coarse Romano-British wares have been picked up, with few shards that can be definitely called earlier. These Romano-British shards are especially abundant over an area near the main road, with numerous pot-boilers, fragments of gritstone querns and shells of *Helix aspersa* (which, as Dr Clay has suggested,[13] appears to have been eaten by the Romanized Britons). This concentration of pottery and cooking débris indicates a habitation site of some sort, possibly a small village or group of huts.

It seems therefore that this group of lynchets was extensively cultivated, though probably not actually laid out in Roman times; possibly it formed part of the cultivated land farmed by the 'villa' on Holt Down to the southeast. Similar Romano-British pottery has been found in rabbit-scrapes in lynchets on Wardown and on the slopes of Gravel Hill Bottom, near this 'villa'.

Connected with the lynchets, and indeed formed by them, is a fine example of an 'interlynchet way', running up the coombe called Hillhampton Bottom. The situation is unusual, for roadways of the Early Iron Age almost always run up spurs rather than 'bottoms'. The sides of this road are formed by the lowest lynchets on each side of the valley, the way itself being sunk between their two banks. It

[13] In his article on 'Wudu-burh', in *Wessex from the Air*, p. 137.

is ten feet wide and the side banks some three feet high. This road runs for about 500 yards northwest up the coombe, but superficial traces of it are lost before it reaches the head of the valley. The rifle range made here (now disused) no doubt helped to obliterate any traces of it.

The Bivallate Ditches

Across the narrow ridge which separates Hillhampton Bottom from the unnamed coombe south of Rake Bottom is a straight ditch between two banks (ditch II on plan), which continues the line of the interlynchet way. It is some 270 yards long and runs between the two 700-foot contours. This 'bivallate ditch'[14] is a good example of a type of earthwork found commonly in Wessex and to a less extent in Sussex. Dr Clay's researches in Wiltshire and the Curwens in Sussex have established the fact that they are commonly associated with tracks or terraceways leading to and through them, and in several cases these tracks are contemporary with lynchets. These investigators have put forward a theory of their use; namely that they were in no sense defensive, but primarily roads: 'that they were cattle-ways along which Celtic men drove their herds in single file from grazing ground to grazing ground, without the danger of the animals running over and damaging the crops that grew in the fields covering the high land'.

There is a second ditch with two banks (ditch I on plan) 500 yards south-southwest of the first, on Hillhampton Down. In its present state it is 350 yards long and cut into at one point by a modern rectangular pond, now dry. Its southeast end is destroyed by modern ploughing, but its course can be traced as a stony band across the field.

The date of these ditches is presumably that of the associated lynchets. In a section cut on Glatting Down, near Bignor, Sussex, Dr Curwen found late Bronze Age pottery in the silting of such a ditch, and in a Wiltshire example (200 yards southwest of the Swallowcliffe Down Early Iron Age village) Dr Clay found a fragment of pottery 'undoubtedly Early Iron Age in date'; it may be presumed that, so long as the downs were cultivated and there was consequently a need for keeping cattle from straying into the fields, ditches of this double-banked type would continue to be made. We have evidence of agriculture in Neolithic times and of lynchets earlier than the first phase of the Early Iron Age, and there is no reason to suppose bivallate ways to belong exclusively to any one period.

[14] This name, though clumsy, has been adopted because it assumes no purpose for the type, which the alternatives, 'covered way' or 'cattle way', do.

BUTSER HILL

The Barrows

On the spur called Ramsdean Down are three round barrows placed in a row running roughly east-west. They all have slight ditches, have an average diameter of 60 feet and are some four feet high. All have been dug into from the top.

On the flat hill-top are two large barrows close together; one, the more southerly, is a simple bowl barrow about three or four feet high with the ditch obscured; it is much mutilated by rabbits, and on their scrapes flint flakes can be picked up. The material of the barrow is the yellow clay-with-flints which caps the chalk at this point. The other barrow was originally a fine bell-barrow, about eight feet high and 135 feet in diameter, but it has been ruthlessly dug into from the top and one side, leaving a gaping crater which disfigures the once symmetrical mound. On the south side the rabbits throw out small fragments of unburnt human bones, presumably of secondary interments.

Near these two barrows is a roughly circular water-hole, which even during the drought in the summer of 1929 was damp at the bottom and had rushes growing in it.

On Little Butser, southwest of Round Copse, is a low mound some 30 feet in diameter and about one foot high. It is probably a denuded barrow. Other similar mounds are to be found among the dense undergrowth on the flat hill-top.

Not far from this barrow is one of those peculiar mounds known as ' pillow-mounds ', which have been described at length in ' *Wessex from the Air* ', pp. 18-24. (A reference to this one will be found on p. 19). It is a low rectangular mound, about nine inches high, surrounded on three sides by a ditch six feet across, measuring on the outside of this ditch 35 feet by 20 feet. The purpose and age of these mounds is still debatable: some seem to have been artificial rabbit-warrens, but this seems hardly likely on Butser, for although the hill swarms with rabbits, which find ample accommodation in the loose clay soil, not one of them has selected this mound for its home.*

About 100 yards north of the barrow and pillow-mound is a circular platform, 30 feet across, levelled on the slope. Around this the

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* I am now convinced that some pillow-mounds at any rate were certainly built to provide accommodation for rabbits. There is a large group of them at Ditsworthy Warren on Dartmoor, and most of them are today full of rabbits. They are made of earth and stone, and are particularly numerous in the immediate vicinity of the Warren House itself.—O.G.S.C.

mole-heaps contain coarse sherds, quern fragments and pot-boilers, pointing to a settlement.

Summary

In our examination of its earthworks we have caught intermittent glimpses of the long history of Butser Hill. We have seen it first as an important meeting place of ancient trackways ; associated with them was perhaps the settlement which grew up near the south spur somewhere about the end of the New Stone Age—say about 2000 B.C. Our next glimpse reveals a new people, knowing metals, who thought the hill worthy to be the burial place of their great ones for whom they made the barrows. Possibly now the increasing traffic cut the track hollow from the valley on the northeast spur. But, for a long interval, until about 200 B.C., we know nothing of its inhabitants ; then, in the Early Iron Age, we see its sunny southern slopes chequered with cornfields, and cattle grazing in the coombes. Perhaps a sudden danger suggested the refortification of the entrenchment on the south spur ; or possibly the work was carried out by some lawless tribe in order to gain control over the increasing traffic along the converging ridgeways. Then the Romans came, and the fields were peacefully cultivated as before : at the foot of the hill on the south was perhaps a little hamlet of thatched and wattled huts where the herdsmen and farm labourers lived. Life went on uneventfully until the Saxons came, and Arthur and his army may have tramped along the ridgeway on their way to victory at the Castle of Guinnion.

The hills were then abandoned, and new villages grew up by the springs at the foot of the Downs. Butser was a hill no longer occupied by men, but only by rabbits and hares. Through the Middle Ages the ridgeway was still a main road with strings of pack-horses plodding slowly along. Meanwhile the Portsmouth to London road became more and more frequented. Samuel Pepys rode past Butser in his coach on his way to Portsmouth on a fine April morning in 1662, and early one day in October 1805, a coach came racing by from Portsmouth, carrying to London the news of Trafalgar. Butser has watched human endeavour for 4000 years : it may watch them for as long again and outlive them all.

My thanks are due to Dr Williams-Freeman for his valuable help in discussing some of the problems on the spot, and to Messrs D. T. Bertram and R. Carter, whose help in the field alone made possible the plans and sections of the earthworks here illustrated.

The Glozel Forgeries*

by A. Vayson de Pradenne

President of the Prehistoric Society of France

THE readers of Antiquity were, from the first, correctly informed about Glozel. But, having exposed the fraud, the Editor decided to ignore the torrent of polemics which ensued. His attitude was a wise one, for a forgery can only be scotched in its own country.

Why then revert now to the subject ? Are there not good reasons for saying no more about it ? One might urge, for instance, that the affair was now at an end : that it was an absurd hoax which now has been recognized as such by practically all prehistorians ; that there remain only a few obstinate dupes who refuse to admit their original mistake ; and that in the heat of controversy, inexpert people have become involved and have taken the affair out of the domain of science : so that it has now ceased to be of interest to serious students—it is no longer Science.

To such arguments we reply :—' Pure science ' is an abstraction. In reality Science consists, at any given moment, of the sum total of the actions and opinions of the men who study it. It is these people who create what is called Science, and its value is exactly proportionate to theirs. Their behaviour, therefore, is by no means a matter of indifference to us, and it needs careful scrutiny. Thus, to use a simile, the idea of *weight* can be conceived quite apart from *scales*. But since in practice it is by means of scales that weights are determined, the physicist must see that his scales are true before discussing the results obtained by their means. He must know what order of accuracy to expect and, if the scales are out of order, he must discover the cause.

To withdraw from a controversy may be expedient as a means of evading attack, but there is absolutely no scientific merit in such

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* Translated by the Editor. It will of course be realized that detailed exposures of the hoax have been published in numerous journals and do not therefore require reiteration in an article of a general character like this. Apart from the original exposure by MM. Vayson and Dussaud, the official reports referred to below give detailed categorical proofs of forgery.—Ed.

weak behaviour. It is merely playing the game of the forgers who attempt to profit by the withdrawal. The history of previous hoaxes shows that an affair of this kind is never completely finished until all the dupes, their disciples and friends are dead. The forger himself rarely confesses, but even when he does one never gets a complete admission of deception from all the dupes. A hoax which has been unmasked and recognized as such in contemporary scientific circles may then be simply put on one side with contempt, but yet if the evidence has not been completely exposed it will reappear at some later date. It comes to life at the first favourable opportunity, which is generally that of the emergence of a fresh hoax.

There are curious resemblances to be noted between forgeries of quite independent ancestry. Should a new hoax spring up, its dupes and advocates will not fail to invoke the aid of analogy. Their arguments are always the same : ' Analogous discoveries have already been made ', they say, ' but they were declared to be false because they constituted a new discovery, because they ran counter to orthodox views. The similarity of the present discovery leaves no room for any further doubt ; the authenticity of both finds is proved '. Thus does one hoax subsidize another.

That is why a hoax has to be as thoroughly extirpated as a crop of weeds ; it is essential to pull it up, roots and all, if one does not want to find it sprouting again some fine day. That is why the Société préhistorique française thought it necessary to take legal action ; so that the hoax might be shown up in all its details before the general public, misled as it was by press stunts, and before those timid prehistorians for whom scientific proof was insufficient and who had not the courage to hold an opinion contrary to that of certain eminent officials.

The history of Glozel is useful as well as diverting, because it lays bare so cleverly the workings of imposture and the development of a controversy. It is a strange human comedy, presenting a group of men of the scientific world and revealing both the positive and negative aspects of their knowledge, their method and their temperament. We have been allowed to see the scales of human judgment and how they behave in action. Each side rallied to its support everything that could possibly help to decide the issue. Thus we can assess the exact intellectual value of those combatants who retired hurt—and this is quite a useful acquisition for future use ; in addition we have seen how an imposture is born, and how it grows and struggles against the truth.

How and why did the impostor come to start operations ? How and why did the dupes fall into the trap ? By what arguments did they then try to bolster up their mistake ? That is the aspect of the case which really deserves to be studied, because it leads to results of general utility in the realm of knowledge.

It must be observed that this concentration of interest on personalities does not imply a loss of objectivity in our treatment. It is as men of science that the individuals concerned are to be judged ; it is from this point of view that they are so diverting. Reduced to the lowest terms of its wretched material content, Glozel would have no interest and the whole affair would be incredible. It is the living subject and its behaviour that is all-important.

A complete history would exceed the compass of this essay. We shall therefore attempt only to sketch the principal events in outline.

On 1 March 1921, a peasant proprietor named Fradin, living at Le Glozet, a hamlet of the commune of Ferrières-sur-Sichon (Allier), when working in a field brought to light some rather peculiar broken bricks. They attracted his attention ; he dug on the spot and laid bare, at a depth of about 3 feet (1 metre) a pavement of oval form consisting of about fifteen large flat bricks. This paved area measured about 7 feet (2 metres 50) long and was enclosed within a little wall of unworked stones and small bricks cemented with mortar. All the interior surfaces had been subjected to an intense heat which had vitrified them. It was an old glass-kiln exactly resembling others which had been discovered in the neighbourhood. Moreover, amongst the débris were found fragments of glass, broken crucibles and a piece of iron which has since been identified as a glass-blower's rod.

Naturally the Fradins—a young man, his father and grandfather—could not explain their discovery. They thought it interesting because of the pretty appearance of the sparkling bits of glass and vitrified objects. Like every peasant on such occasions, they proceeded to hunt for ' the treasure '. Failing to find it, they informed the village schoolmistress, who, in accordance with the instructions issued to all such, made a report to the Educational Inspector of the department. She thought it consisted of a cremated urn-burial. The Société d'Emulation du Bourbonnais obtained information of the report, and delegated its nearest member, M. Clément, schoolmaster of La Guilleymie, to undertake a preliminary examination. M. Clément was young and had only very rudimentary ideas about archaeology.

After his first visit on 9 July 1924, he accepted the burial hypothesis, and those members of the Société d'Emulation who visited the site a few days later equally failed to recognize the true character of the discovery.

If they could have said to the owners : ' This is merely an old glass-kiln of no interest', no doubt the matter would have rested there. But their uncertainty suggested that the find was something new, unknown, mysterious. It might be hoped then that from it would emerge objects of a remarkable character, of great *value* . . . Imagination had free rein and the field was clear for imposture.

The Birth of the Hoax and the Way it Developed

Young Fradin's curiosity was aroused and he continued to dig. He was a humble peasant about 18 years old, somewhat work-shy and with a rather morose expression of countenance, anxious to escape from the hard labour of the farm work which was rather beyond his physical powers. He had done well at the primary school and remained there longer than usual, and he had developed artistic tendencies ; his bedroom was ornamented by little watercolour paintings done by himself.

Schoolmaster Clément paid regular visits to Le Glozet. Attracted by prehistory, he was glad to find in young Fradin an attentive and keen pupil. He showed him his small collection and the few books and pamphlets he possessed. In the collection was one of those lumps of schist, quite common in the district, which are by-products of the manufacture of bracelets of the Bronze or Early Iron Age. This lump had the peculiarity of being engraved, at some period unknown, with four signs, and was doubtless carried as an amulet. The first sign was like an arrow, the three others like the letters S T X. F. Pérot, an old collector of Moulins, had published a note about it, in which he described also a diorite axe with what he believed to be a cross and a kind of Greek *lambda*. Apropos of this, Pérot referred to the existence in ancient times of markings in the form of a cross, a *tau*, a swastika, etc. Clément accordingly regarded these objects as possessing quite a peculiar interest. A layman who saw them might well think that such interesting objects would be very easy to reproduce. This time the temptation was direct.

In a letter of 13 October 1924, addressed to the President of the Société d'Emulation, Clément narrated that young Fradin had just forwarded to him as having been found near the 'grave', the end of a

schist pebble with three engraved signs : almost S X T (the lower part of the S was unfinished, the T was placed on its side). This was the first Glozelian object.

No shadow of doubt obtruded itself upon the schoolmaster, who was delighted by this discovery. He has since recalled the extreme hesitation with which it was offered to him.

The timid attempt had succeeded; it was not repeated, however, until two months later. This time it was the axe which inspired the forger. In a letter of 31 December 1924, Clément informed the Société d'Emulation of young Fradin's discovery of a ' piece of a rough polished axe of black schist '. He asked for a grant of 50 francs to continue the excavations.

The Society refused ; doubtless the interest of the finds appeared inadequate. Immediately there appeared ' further remarkable objects ', announced by Clément in a letter of 30 January 1925. He explained that a ' closer examination ' of one of the original bricks had revealed signs on it, of which he enclosed a copy. It was young Fradin who had drawn attention to this brick, whose surface was covered with mud. Clément, who had already seen all these bricks—about fifteen in number —without noting anything, was surprised now to find one of them covered with signs ; but he was so devoid of all suspicion that the possibility of fraud never occurred to him.

The forger's inspiration for this first inscribed brick appears to have been again derived from Pérot's notes and from a book of Levistre's lent to Fradin by Clément ; in this were figured certain signs which, engraved on megaliths, were more or less fortuitous in character and belonged to an unknown period. Levistre regarded them as a kind of script.

The Société d'Emulation was intrigued and its suspicions were aroused by this inscription. M. Espérandieu was consulted and he pronounced that ' it is Latin . . . or it is a forgery '. Doubtless the forger realized the danger, for he did not publish another of the kind, and confined himself during the months which followed to the manufacture of one or two small schist axes with some engraved signs.

It had taken more than a year to get to this point. Such moderation at the outset was undoubtedly one of the factors which contributed largely to the success of the hoax. The forger was prudently and patiently sharpening his native peasant wit. Thus he made up for his ignorance and lack of technical skill ; he was able to find out gradually how to act and how best to avoid suspicion.

Apart from these ' merits ' on the part of the forger, the hoax was well served by the inexperience of the first dupe. M. Clément could not detect the imposture, gross though it was, from a mere examination of the objects. The suspicion of fraud, which should always be kept in mind, did not occur to him. He acted imprudently, moreover, in lending books which served to guide the forger.

But the great development and success of the hoax is undoubtedly due to Dr Morlet. A doctor with a small country practice who had recently come to live in Vichy, Dr Morlet carried a light cargo of archaeological knowledge. He had done some excavating on a Gallo-Roman site, and had once attended Girod's lectures on prehistory. Aided and abetted to a remarkable extent by his weaknesses, he committed almost all the mistakes that could be committed in such an affair. Accordingly his case is peculiarly instructive.

Dr Morlet had heard of the excavations of Le Glozet and of the refusal of a grant by the Société d'Emulation, and he told M. Clément that he would like to see the finds. M. Clément showed him the objects and introduced young Fradin. Being quite incompetent in these matters, the good doctor did not detect the forgeries; on the contrary, he considered them very interesting. Full of enthusiasm and imprudence, he declared on the spot, so Clément has narrated, that ' here was a discovery which would attract to Le Glozet the savants of the whole world, more compelling than was even the Java skull'. A wire fence should be put round the field and a charge made for admission. Young Fradin listened agape. . . . He must have seen at once that Dr Morlet was his man, and he was not going to let him slip. But he acted with no undue haste.

It was not till a month later that Fradin, so he tells us (*Mercure de France*, 15 August 1926), sought out Dr Morlet at Vichy, to ' tell him his troubles'.

Indignant at the Society's refusal of a grant, Dr Morlet promised the Fradins, so he told me, that he would give 200 francs instead of the 50 francs asked for, and that '*if more was found he would give more*'. There he committed the classic error which cost Boucher de Perthes the forgery of Moulin-Quignon. Two days later there was brought to him a sort of pot-base, made of badly puddled clay, inadequately fired. It was the birth of Glozelian pottery.

His behaviour to M. Clément, who, after all, had been concerned from the outset with the pseudo-site, was somewhat brusque, to say no more. He arranged to cut him out, and to take over from him the

original discoveries. He signed an agreement with the Fradins, purchasing the right of excavation, and the scientific control of the discoveries. The Fradins were to retain actual control over the objects themselves, thus obtaining the right of being constantly present at the scene of action to supervise all the finds.

From this time onwards it was Dr Morlet who excavated at Glozel, always in the company of young Fradin. Being, however, often absent himself, he allowed his ' colleague ' to excavate alone. Furthermore, he began to increase his own knowledge by reading archaeological books which he passed on to his associate. ' I have never concealed anything from my colleague ', he told me proudly.

This collaboration became increasingly productive, leading up to the great scene of June 1927. But we must not anticipate. What we must bear in mind at this stage is the fact that Dr Morlet, by his incompetence, rashness and lack of judgment, gave the impetus to a hoax that otherwise would have miscarried at the outset.

In my ' Chronology of Glozel ' (*Bulletin de la Soc. préh. franç.* 1927, XXIV, 293–319) I have given the later history of the forgeries. This was made possible by M. Clément's letters, kept by the Société d'Emulation. Dr Morlet's first publications inform us of the course of events immediately after his arrival on the scene. From these unassailable documents we learn :—

1. That the different classes of objects appeared in succession ;
2. That the technique of manufacture gradually improved (this is particularly noticeable of the pottery) ;
3. That the forger's output corresponded closely
   with the documents provided by his dupes
   with the wishes they expressed
   with the objections and criticisms of opponents.

This development is true to type ; it is a characteristic of all great impostures. The forger is guided step by step by his dupes, thanks to their confidence and to the discussions which they initiate.

The Triumph of the Hoax and its causes

To have thus succeeded in deceiving a young schoolmaster and a country practitioner of no special attainments was but a moderate achievement. The forger was to have a real triumph, thanks to a series of events which we shall briefly narrate.

The jealous nature and vanity of Dr Morlet prevented the so-called site from being properly tested and examined by prehistorians. At the outset Dr Capitan had visited the newly discovered site, and it must be admitted that he did not assess it at its true valuation. However, his intervention would have brought the site to the notice of prehistorians, and it would not have been long before it was shown up. But Morlet feared that Dr Capitan would ' steal his thunder ', so he showed him the door. He would share his good fortune with no one, and incompetent though he was, he rushed into print with his ' Nouvelle station néolithique ' (September 1925), without consulting any specialist, without making any attempt at serious study. So far from approaching people who might have enlightened him, Dr Morlet passed over the Société d'Emulation, of which he was a member, and paid his court to local journalists, in order to advertise himself.

But pamphlets and newspaper articles have no echo. So Morlet went to Paris, and knocked at the doors of a number of savants : M. Boule, Professor of Palaeontology at the Museum ; M. Dussaud, one of the conservators of the Louvre ; M. de Mortillet and the Abbé Breuil ; M. Camille Jullian, the historian of Gaul ; and M. Salomon Reinach, Conservator of the Museum of National Antiquities and a prolific writer. Everywhere he himself was received with cordiality, but his finds with reserve.

Confronted with this reserve, these warnings of the scientific world, anyone else but Dr Morlet would have paused to reflect, would have verified his facts, called in assistance and eventually discovered the truth. But not he! Oblivious to all else in his pursuit of fame, he could see only a hostility over which he must triumph at all costs. He managed to persuade—and there are many forms of persuasion—a publicist of no great repute, M. van Gennep, who supplied the *Mercure de France* with a running commentary on ethnography, folklore, etc. Van Gennep opened the *Mercure de France* to Dr Morlet, and from then onwards this remarkably omnivorous literary review became the official organ of Glozel. I have described in detail elsewhere the superficiality, the astounding one-sidedness of van Gennep's first Glozelian articles, reporting without any attempt at verification the sayings of the Fradin family. Since these included a certain number of stories harmful to members of the Société d'Emulation, contradictions and corrections were not long in appearing. This was the beginning of a controversy of a personal kind.

In his infectious enthusiasm, backed by an obvious sincerity,

in the money that he never stinted for the cause, by his personal friends, and in the lure of mystery and novelty so dear to the laymen, Dr Morlet possessed an armoury of weapons with which to capture the journalists. So he had widespread support in the press.

Thus the protagonist of Glozel prevented that atmosphere of mistrust, present from the outset in scientific circles, from spreading to the general public. A fortunate accident obtained him converts amongst persons who opened the doors of the Institute to the Glozel hoax.

The forger had been lent textbooks of prehistory, and he had copied, more or less fancifully, and without discrimination, objects belonging to all ages between the palaeolithic and the historic periods. The result, a regular hotch-potch, included reindeer engraved on pebbles, polished axes, pottery reminiscent of Hissarlik and inscriptions. Amongst the last were some actual Phoenician signs—included to please Dr Morlet, who, from the start, had tried to discover traces of Phoenician amongst the scrawls in which he wished to find the beginnings of the alphabet. Since not only were the objects strictly associated, but the same inscriptions figured alongside of engravings of reindeer and on vessels of the Hissarlik type, a mingling of different periods was inadmissible. Dr Morlet, refusing to admit fraud, placed the whole group in the famous *hiatus*, whose transitional character and mystery have always conspired to make it the natural dumping-ground of forgeries. But in the present case this involved two conclusions as important as they were bizarre :—both the survival of the reindeer and the appearance of the alphabet in France at the beginning of the neolithic period.

By a stroke of good luck for the forger these results, contradicting the strongest and most abundant scientific evidence, coincided remarkably well with the opinions of M. Salomon Reinach on the Mirage Oriental. Dr Morlet perceived this, developed the idea and sent to M. Reinach (who, well advised at the outset, had not yet allowed himself to be caught) his third pamphlet, inscribed : ' To the father of the Mirage Oriental, an unrecognized child '. An overwhelming temptation this for a naturally obstinate man, obsessed for forty years by a theory constantly in conflict with the facts ! Salomon Reinach set out for Glozel. He was lost before he started, for to counterbalance so great a temptation he would have required a highly disciplined critical faculty. Now this great man of learning, whose brain is essentially receptive, has always conspicuously lacked this faculty. He has,

moreover, always looked at archaeological facts only through a veil of literary form, and he has always neglected the study of *things* and of technical matters ; so that it is not hard to perceive why he has so often been the victim of forgers. (For his mistake over the tiara of Saitaphernes, though the most famous, is but one of a long series).

Reaching Glozel 24 August 1926, M. Reinach saw unearthed in front of his eyes, by the expert hands of young Fradin, a complete series of the best products of the locality. M. Seymour de Ricci, who accompanied him, in vain tried to put him on his guard ; the innocent air of the excavator, the ardent faith of Dr Morlet and the yielding nature of the soil definitely overcame him. One must read in the *Mercure de France*, 1 November 1926, Dr Morlet's naive and delicious account of this excavation from which the old Conservator of Saint Germain, happy as a child, found, one might almost say to order, what he had announced that he wished to find. Two days later M. Reinach ascended the tribune of the Académie des Inscriptions and said that he could ' state without hesitation that all these objects are authentic, have not been tampered with and are from the same site ', and that the theory of a mystification ' is for the future untenable '.

From this moment it was like a game of ninepins. The fall of Reinach brought with it automatically that of Espérandieu, his creature at the Académie des Inscriptions.

Depéret next hastened to the spot. Dean of the Faculty of Science at Lyon, his life had been dedicated to the study of palaeontology and geology ; but a few years ago he tried to found and direct a school of prehistoric and anthropological research that should be worthy of the former capital of Gaul. Hearing of the success of a discovery made within his own region, he came post haste with a colleague (14 September 1926) and allowed himself to be convinced. His researches in palaeontology and geology gave him no qualifications in prehistoric archaeology, least of all with regard to forgeries. On 11 October he announced before the Academy the authenticity of the discoveries. His influence was considerable for two reasons. He had quite a following of colleagues, pupils and friends, so that numerically he counted for a good deal. Then again, being well known as a geologist and as an excellent palaeontologist, he was nothing if not a man of science. Many people who attached no importance to the opinions of MM. Salomon Reinach or Espérandieu, were influenced by a statement from Depéret. It was in this way that the Abbé Breuil himself, whose visit to Glozel was made under unfavourable circumstances,

allowed himself for the moment to be drawn into a qualified admission of authenticity. About the same time (19 to 23 October 1926) the old Celtic scholar, J. Loth, of the Académie des Inscriptions, having attended the excavations, declared himself convinced.

At the end of 1926 the situation was definitely in favour of Glozel. A constellation of members of the Institute had stated from the tribunes of both Academies that it was one of the greatest archaeological discoveries of the century.

By the side of orthodox Glozelians, who recognized the neolithic authenticity of the finds, a small schismatic group formed itself round M. Camille Jullian, the learned historian of Gaul, regarding them as Gallo–Roman. M. Jullian described to the Académie des Inscriptions how he had succeeded in deciphering the Glozel bricks, which were inscribed, according to him, in Latin. He saw in them the magical formulae of a 3rd century sorcerer's workshop. It should be remarked that he created a feeling of amazement and that no epigraphist followed him. However, his solution recruited a few supporters from amongst the moderates, because superficially it seemed to harmonize everything : the pottery, the crucibles, the glass, etc., were not at all inexplicable in the Gallo–Roman period ; the miscellaneous assortment of polished axes, of uncouth inscriptions, of animal drawings were to be accounted for by magic. And Count Bégouen, Professor of Prehistory at Toulouse, who some months before had called for the appointment of a committee to test and investigate the site, had reached the point of stating in his lectures, at the beginning of 1927, that he was beginning to decipher some Glozelian inscriptions himself. (*Le Télégramme*, 12 and 21 Feb. 1927).

However, M. Jullian could not reconcile himself to the progress of the Glozelian script when it veered steadily round towards Phoenician. Being no longer able to read them as Latin (as he had succeeded in doing with the first formless scrawls) he declared that the first productions of 1926 were forgeries. This had two results : it aroused the fierce opposition of Dr Morlet and of M. Salomon Reinach ; and it subsequently induced the forger, who respected the criticism of so eminent a man, to correct his later products.

Discovery of the Hoax

In spite of the apparent triumph of the Glozelian theory, in spite of widespread reticence due to the lack of arguments strong and precise enough to be set against the vehement statements of highly-placed people, a latent scepticism continued to smoulder amongst those most

closely in touch with prehistory and epigraphy. Thus M. Dussaud, speaking at the Académie des Inscriptions in 1926 about the oldest alphabets, made no reference whatever to Glozel. But in the history of an affair like this, one must not reckon only with official public pronouncements. The earliest statement of scepticism published is, so far as I am aware, that of M. A. de Mortillet, who, at a meeting of the Société préhistorique française of 23 November 1926, stated that there were forgeries at Glozel, and that the letters of the inscriptions appeared to him to be ' suggested by various ancient alphabets, of different dates and origins, with the addition of imaginary signs '.

Mr Crawford was the first to write a note on ' l'affaire Glozel ', which appeared in the first number of ANTIQUITY (March 1927). This note was reinforced and expanded in an article which appeared in the June number, where it was concluded that ' the majority of the objects of Glozel were quite certainly forgeries '.†

The same month (June 1927) without knowing about Mr Crawford's articles, I went to Glozel at the suggestion of MM. Boule and S. Reinach. Since for the previous two years or more I had neglected my prehistoric work, I was out of touch with the affair. The first rapid examination revealed to me the obvious and typical spuriousness of certain objects. Thinking that there might be, as so often, a mixture of spurious and authentic, I attempted to discriminate; I observed with surprise, however, that everything was spurious and obviously the work of one individual, with the exception of the bricks and crucibles. On the bones and polished stones could be seen traces of metal tools, and on some were clear marks of a file. The pottery, which was hardly baked at all, displayed an utter ignorance of all technique ; it was the work, not of primitive man, but of a child ; the incised designs, in spite of a clumsy and obvious faking of the surface, showed on certain specimens the traces of metal implements. It was, moreover, quite impossible, in view of their lack of firing and fresh condition, that they could have lain for centuries at a shallow depth in the damp soil of the locality.

A single rapid technical examination sufficed therefore to form a conclusion. I said nothing to the Fradins, but without further delay

† In using the expression 'majority' I wished to be strictly accurate. The only 'genuine' objects in my opinion, were, as I said, the bricks, crucibles and glass fragments and other débris from the glass kiln, together with a few minute flint chips. All except the last were plainly of quite a late date, and irrelevant to the main issue. I never had the least doubt that the rest of the stuff was all of it forged.—TRANSLATOR.

I wrote a preliminary note recording these observations. (*Bulletin de la Soc. préh.*, July 1927).

Shortly afterwards I called upon Dr Morlet, satisfied myself that the objects in his collection were by the same hand as those in the ' Musée Fradin ', and with his permission made two excavations at Glozel—the first time in his absence, the second time with him. It was a case of scratching at the sides of shallow trenches already dug (about 70 centimetres deep). Some small objects were met with in clay mixed with granitic grit, with no clearly visible traces of their method of insertion there. Towards the bottom of the trench, where the clay was more compact and harder, I found a sort of horizontal tunnel by means of which an engraved pebble had been intruded. The clay which had been used to stop up the hole again was of a consistency quite different from that of the rest.

A trench quickly driven out at right angles showed that a block of between 20 and 30 centimetres only (8 to 12 inches) in width had been thus ' salted ' : beyond this one ceased finding anything. This proved that the ' salting ' of the site had been carried out *pari passu* with the progress of the previous excavations. The loneliness and isolation of the spot made this easy. The prying eyes of neighbours could achieve nothing, for they would not be able to tell from a distance whether any one was digging to extract objects or to insert them.

I was also able to satisfy myself that two recently opened pseudo-tombs were a kind of dug-outs set with dry stone walling. The roof was made of two inclined slabs. The forger had not even taken the precaution of ramming soil into his erection, and there were empty spaces left between the lateral stone walls and the earth which they were supposed to be supporting ! The purpose of these constructions was to make possible the discovery on a single occasion of a large collection of vases and bulky objects which it would have taken too long and been too risky to insert one by one in the ground. I explained his mistake to Dr Morlet ; but he refused to be persuaded, and launched out at once into that course of violent language and writings which he has pursued ever since. I set forth all these facts in detail in my second note which appeared at the same time as the first one (*Bull. de la Soc. préh.*, July 1927).

My statements and my offer to re-enact all these proceedings upon the spot were met by M. S. Reinach, verbally, with absolute confidence and unshakable faith. His tactics in front of me were not to engage in a public discussion.

To complete my investigations I then took up the history of the affair. Everything then became clear, and I published a long note entitled ' La Chronologie de Glozel ' (*Bull. Soc. préh. franç.*, September 1927). There I set out in detail the origin of the hoax and the stages of its evolution—the perfecting of technique, the emergence of discoveries in successive groups, and the influences affecting the forger, as I have just shown above.

Whilst this note was in the press, M. Dussaud undertook the task of demonstrating to the Academy the spurious character of the inscriptions and of the whole site of Glozel. He began at a secret sitting held on 16 September 1927. But the secret soon leaked out, and two days later the *Journal* published a long article on the subject. The character of the arguments used by M. Dussaud are essentially of an epigraphic kind; they can be read in his brochure : 'Autour des inscriptions de Glozel'. The most piquant portion is that which deals with the evolution of the inscriptions. To the composite, shapeless scrawls of the early days there had succeeded an incoherent mixture, consisting of true Phoenician letters but belonging, curiously enough, to the latest phase of that language. As soon as this anomaly was pointed out, there immediately appeared a few letters of the oldest phase. Finally, in the last months of 1926, Dr Morlet, and through him, the forger, having heard about the quite recent discovery of the alphabet of Ahiram, four centuries older still, the writings of Glozel began to take on a likeness to that of Ahiram.

The Controversy

After such demonstrations one might have thought the matter ended. To do so would be to underestimate the capacity for obstinate resistance displayed in such cases by those who have been humbugged. The Abbé Breuil, however, whom the advocates of Glozel were so proud of including in their ranks, wrote me a letter (2 August 1927) which I published forthwith, in which he explained that it was solely on the authority of his precursors at Glozel that he had provisionally admitted the hypothesis of authenticity, in spite of his private scepticism.

But there remained a small group of irreconcilables, composed of those who had committed themselves over-rashly to the affair, amongst whom, it should be observed, was not a single student of prehistory.

First of all there was Dr Morlet, who in pursuit of fame had, so to speak, staked his whole existence on the wretched card dealt him by

young Fradin. Glozel, true or false, was for Morlet ' to be or not to be '. Still young and endowed with a wild energy, with an inflexible resolution and a naive and immoderate self-esteem ; devoid, on the other hand, of competence and of the critical faculty, he threw himself whole-heartedly into the fray. A strange figure indeed, for whom one cannot help feeling pity, as for a bull thrust into the arena whose courage and vigour one admires in spite of its stupidity.

By his side was Salomon Reinach, the man who of all men of learning shows the greatest pertinacity in adhering to an error; the man who, in the case of the famous tiara of the Louvre, even after the proofs piled up by the enquiry and the confession of the forger himself, still maintained, in a minority of one, that the problem was ' by no means settled ' [nullement éclaircie]. (*L'Anthropologie*, 1903, pp. 361–4).

These two tireless henchmen were supported by several others. The most influential, for reasons we have stated, was Depéret, the geologist. To his prestige as a man of science there were added the qualities of urbanity and obvious moderation, so conspicuously lacking in his fellow combatants. Instead of maintaining, like them, an attitude of unshakable faith, he freely stated that he was prepared to admit deception if scientific proof were given. The severe rebuffs successively encountered by Glozel failed to draw from him prompt and heated rejoinders like those supplied by Dr Morlet, Salomon Reinach, and others. He said nothing, and one thought ' this time surely M. Depéret has got the scientific proofs he demands ; he must now be convinced and surrender '. But a fortnight or a month later M. Depéret emerged afresh from his silence, and, while not replying directly to the proofs of fraud supplied, expressed yet once more, in describing some new investigation, his scientific adherence to the authenticity of Glozel.

It would take too long to follow in all its windings a controversy which lasted two and a half years. We shall be content to indicate its main psychological features.

The principal feature of the Glozel controversy has been its habit of continually taking up a fresh position. In this it is true to type. The defenders of a false position abandon in turn each of those points whose untenability has been proved, to take up their stand on another. True, in abandoning them they do not admit the truth but cover their retreat with any sort of rejoinder that can be furbished up. Thus there arises a controversy, which is all to the advantage of the protagonists of error. Actually in a scientific discussion arguments are weighed,

whilst in a controversy they are counted ; an objection must not be left unanswered, but the kind of answer given does not matter. Thus the public demand for logic is satisfied. But to achieve this one must go beyond the radius of scientific circles. One has got to appeal to the general public. This is what the backers of a hoax do always ; the champions of Glozel followed suit. We saw how, at the start, Dr Morlet launched his discovery in public by writing to a literary review and to the newspapers. The whole controversy has been organized and backed up by the press. In addition to the time-honoured method of obtaining press support, the partisans of Glozel had certain factors on their side. There was, first of all, the lure of curiosity and novelty ; they had something sensational to provide and newspapers like that. Then Dr Morlet himself, with his ardent faith, his whole expression radiating the zeal of a missionary, stood, from a psychological point of view, for a great power of persuasion. His sincerity appeared to be above suspicion, and the average journalist and the man in the street concluded that there could be no forgery at Glozel.

Thus with the aid of the press it was easy to bamboozle the public —a manoeuvre that in such cases is made easier by the fact that the public is incapable of reasoning, or does so with its heart and not its head. One has only therefore to play upon its emotions.

After my first notes; Dr Morlet's first move was to publish a long personal diatribe entitled 'Sherlock Holmes à Glozel' (*Mercure de France*, 1 August 1927). He added some remarks in a similar strain addressed to Mr Crawford, and M. de Klercker, whose scepticism he had noted. Conducted thus upon a personal basis, a discussion has several advantages for the protagonists of error. It is a huge field with room for every kind of manoeuvre; if one's opponent does not reply he is assumed to be silenced ; if he replies he becomes involved in controversy. The public concludes that one has ' lost one's scientific calmness ', and that it is impossible to discern the truth. That is a satisfactory result for those who are in the wrong.

But the champions of Glozel were able sometimes to obtain even better results by appealing tactfully to public feeling. They appealed to notions of fair play and democratic sentiment by urging that a cabal had been formed against a poor peasant and a country doctor by rich and powerful people, jealous of the good fortune and fame acquired by these humble folk. They truckled to popular snobbery and respect for established positions by declaring that the great authority of MM. Salomon Reinach, Loth, Espérandieu was a sure safeguard against so gross an error.

Then they fed the public appetite for learning 'secret history' by launching explanations that would 'explain all'. I had attacked Glozel, they said, because someone had refused to sell me the finds; M. Dussaud denied the authenticity of the neolithic alphabet, because if admitted his career as an orientalist would be finished. The opposition to the great discovery could be explained in a general way by its novelty, which upset old-established ideas, and by its importance, which aroused jealousy.

To this row of sentimental arguments the Glozelians added others of what is called a 'commonsense character', such as are always trotted out on these occasions—always the same and always false. 'The forger must surely be a great savant, a regular genius, to have organized such a fraud. A poor peasant could never have done all that'. But such an argument passes over the essential fact:—the hoax evolved itself gradually; it was unconsciously directed by the dupes themselves, and its course was guided by the criticisms of its opponents. It was the offspring of collaboration. Then, in the second place, it was maintained that 'there are thousands of specimens; surely a forger could never have achieved such mass production'! Actually, the mass of the specimens did not demand a great deal of work to produce, and in a few months a forger could well turn out a large quantity. Lucas alone in six years forged more than 27,000 autograph manuscripts. Moreover at Glozel, amongst the 1500 or 2500 objects the greater number were fragments of crucibles, bricks and such like from the glass kiln.

The Chief Investigations

(1) *The provisional scheduling.* Directly after the epigraphic proofs provided by M. Dussaud, in consideration of the feeling aroused by the first discussions, the Minister of Education issued a preservation order, dated 5 October 1927. The immediate effect of this was to protect the site as a 'scheduled historical monument' for a period of six months, after which and upon the recommendation of the Fine Arts Commission, the scheduling is, or is not, confirmed. M. Peyroni, curator of the Les Eyzies Museum and M. Champion, the chief of the technical staff of the National Museum (St. Germain) were appointed by the Minister to supervise the site and catalogue the specimens.

(2) *The International Commission.* At the meeting of the Amsterdam Congress, the general assembly of the Institut International d'Anthropologie decided (24 September 1927), on the motion of

MM. Comte Bégouen and Mendés-Corréa, to suggest sending a committee of investigation to Glozel. Dr Morlet let it be known that he ' accepted without reservation ' this committee which ' offered only objective guarantees '. Nominated forthwith by the office of the Institute and composed exclusively of those who had taken no part in the discussion, it consisted of seven members : M. Hamal-Nandrin (Belgium), M. Absolon (Czecho-Slovakia), Miss Garrod (England), MM. l'Abbé Favret, Forrer and Peyroni (France), M. Bosch-Gimpera (Spain), M. Pittard (Switzerland).

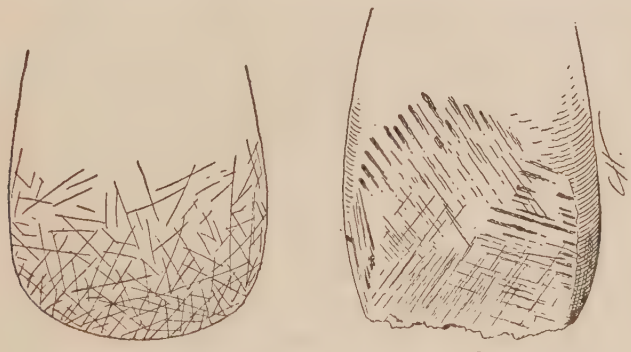
The commission went to Glozel at the beginning of November and excavated for some days, maintaining absolute silence with regard to its proceedings. The journalists present, observing specimens emerging from the ground and the unbounded delight of Dr Morlet, lost no time in announcing that the authenticity of the site had come to be admitted. The commission's report, long and detailed, appeared in the Revue d'Anthropologie (24 December 1927). It denounced the falsity of objects of every class, the recent construction of the tombs etc. ; even the way in which the ground had been salted was recognized. Beneath a badly replaced lump of turf, the commission encountered a cavity full of freshly disturbed soil, at the bottom of which was an inscribed brick.

The commission reported unanimously upon the modern character of the documents which it had investigated. But the champions of Glozel, far from admitting defeat, reacted violently. M. S. Reinach, by way of a reply, communicated the same day to the press a little manifesto which he got two of his Academy colleagues to sign as well. It is a choice incident in his career, full of mistakes though it be : ' The admirable discovery of Glozel lacked only' he said 'the highest blessing of all—that with which the Roman inquisition honoured the genius of Galileo. To this extent the Bégouen commission has deserved well of Science, and the loyal soldiers of a just cause owe it their thanks. As for the commission itself and its originator from Toulouse, they will share with the Commissars of 1633 the only immortality they deserve, that of ridicule '. (Signed, S. Reinach, J. Loth, Espérandieu).

3 *The Champion report.* Shortly after the commission's report appeared, M. Champion, who was sent to Glozel officially to draw up an inventory of the objects, produced a report entitled ' Observations techniques sur les trouvailles de Glozel ' (Nourry, 1928). In a very precise and clear manner (thanks largely to his excellent drawings) the learned technician laid bare the true character of the workmanship

observed on the stone objects from Glozel. The stone in question was generally of a soft nature. The perforations had been made with a steel drill, the sculptured designs with a steel graver of round section (doubtless made from a broken knitting needle). The shaping and polishing had been done with rasps and files, the marks of whose teeth could be seen perfectly clearly on certain specimens. One could even see them on the photographs published by Dr Morlet.

4. *Decision of the Commission on Prehistoric Monuments.* This commission was appointed by the Direction des Beaux Arts, and consisted of MM. Boule, Capitan, C. Jullian, Henri Martin, Verneau, and others, and reported that ' inasmuch as the site did not seem to possess any prehistoric features, they unanimously recommended the Minister not to confirm the provisional scheduling' (10 February 1928).



1
Cutting edge of neolithic axe showing nature of polish-marks

2
Cutting edge of forged axe showing marks of metal file

(*After* Champion)

5. *The Research Committee* (*Comité d'Etudes*). The signatories of the ' Galileo manifesto ' and Dr Morlet thought fit to call into existence a ' Research Committee ', to counterbalance the effect of the International Commission's report and to consolidate the ranks of their supporters. From 11 to 14 April 1928, a dozen people, including S. Reinach, J. Loth, Depéret, etc., came to Glozel and ' were present at ' excavations made by workmen in the clay, and announced themselves ' formally convinced that the finds clearly belonged to the beginning of the neolithic period, without any admixture of later objects '; and they published a report.

That cynical observer, René Benjamin, has taken the curious comedy as the subject of a witty book that has had a great success (*Glozel, Vallon des Morts et des Savants*).

6. *The Legal Actions.* The newspaper '*Le Matin*', captured by the Glozelians as the outcome of some excavations (crowned with success) carried out by its reporters, published a letter of M. Dussaud's which was not intended for publication, in which the learned scientist formally accused young Fradin of being the forger of the inscriptions. Then the great daily caused him and his grandfather to bring an action for libel against M. Dussaud.

The affair thus reached the law courts in a most paradoxical form, with the Fradins in the rôle of plaintiffs and accusers. The action could lead to no good result; the only question of which the court could take cognizance was that of libel and not that of its foundation. It was to be feared, on the other hand, that the public would regard a conviction for libel as a proof of authenticity. The Société préhistorique considered that, to vindicate the honour of its subject, it should put a stop to a scandal which disgraced French science. It felt bound to adopt a course of action which should open all eyes to the truth and to arrange matters so that the Law itself should unmask and punish the fraud.

The matter was legally possible. In fact, though there had been no sale of objects except to Dr Morlet, one could denounce as felonious the act of taking money from visitors for exhibiting modern productions in the guise of antiquities. Actually a well-grounded accusation of this kind would lead to the issue of a search-warrant and to an official enquiry by the police authorities. One might hope by this means to obtain for the public decisive proofs of a different order from those already furnished—proofs, moreover, which the champions of Glozel could not set aside as the outcome of professional jealousy and so forth. This result was, in fact, actually achieved.

The secret of the decision was well kept. Maitre Maurice Garçon, legal adviser of the Société préhistorique française, arranged all the formalities, and on a charge made on 24 February 1928, before M. Python, magistrate at Moulins, a search directed by Dr Regnault, then President of our Society, was made the following day at Glozel. There were found the débris of manufacture, and a certain number of objects were seized and submitted to the expert examination of M. Bayle, Director of the Service d'Identité judiciare at Paris.

7. *The Bayle report.* M. Bayle adopted methods both exacting and thoroughly scientific. In many criminal trials of the past one had watched him unravel painful mysteries. His knowledge was profound, as should be the knowledge of a man upon whom the life of an accused person may depend.

M. Bayle began by a general examination which quickly led to the detection of certain evidence of modern fabrication in the objects submitted to him. He proceeded to an exhaustive examination of all this evidence. At the request of the magistrate, M. Bayle sent to him in the spring of 1929 the only portion of his report which was finished —that dealing with the inscribed bricks. The gist of his report has been published.

M. Bayle affirmed that the bricks had not been baked and that they dissolved at once on being placed in water ; that some of them had never been buried at all ; that they contained as impurities tiny fragments of modern origin, particularly strands of cotton and coloured by aniline dye, pieces of moss and grass still retaining chlorophyll, etc.

Thus, by methods which excluded all archaeological considerations, the learned specialist succeeded in triumphantly proving fraud.

It may be said that the publication of this report brought down the curtain on the farce of Glozel. The great dailies and the general public are no longer interested in it ; and since the scientific world had already made up its mind long before, the affair was left without any genuine support.

The Present State of Affairs

Last September, before finishing his report, M. Bayle was assassinated. He was a martyr to his profession and fell as the victim of one of those criminals whose misdeeds he had frequently shown up by the methods of his department.

At the present moment the action brought by the Société préhistorique is being delayed until M. Bayle's colleagues have finished drawing up their report. But, as we have already said, the result is now a foregone conclusion ; correct opinions have been formed and so far as this is concerned the desired object has been attained. There remains little more than the punishment* of the offender.

True, out-and-out Glozelians will not then, and never will, admit their mistake. But their ranks are strangely depleted. M. Depéret is dead ; the two pupils who fought by his side abandoned their Glozelian positions after the Bayle report. Certain of the Glozel champions who were noted for the violence of their statements, and their impertinence to those who contradicted them, appear to have admitted the truth.

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* In view of the facts already stated one cannot help hoping that this may not be vindictive.—Translator.

But, being doubtless reluctant to submit to the humiliation of publicly confessing their error—a course which would involve also the withdrawal of their ill-judged remarks—they are content to remain silent.

As for Dr Morlet, he has attained Nirvana, where no event has power to disturb his serenity ; and he passes through life with his eyes fixed on the Glozelian paradise ! Science rejected the Truth he offered it, so he appealed to the great heart of the People; but the People has failed him. He appeals now to future generations ; and then, surely, at the last there will come a day. . . .

Outside the ranks of orthodox Glozelians, clinging to the 'neolithic' hypothesis, there is a group of heretics. Each has his own solution to offer and each can translate the famous tablets. M. Camille Jullian continues to read some of them as a cursive script of low Latin. Lt.-Col. de St. Hillier, a retired African officer, translates them all in the light of Phoenician and of Arab roots. He has even published a 'Glozelian grammar for general use' (*Petite Grammaire glozélienne à l'usage de tout le monde*, Moulins, 1927). The Dutch Pastor Voelter deciphers Fradin's bricks as Hebrew, and has just published a huge volume on the subject (Strasbourg, 1929). M. J. Celajor, a Spaniard, explains the Glozel inscriptions by means of Basque! M. Cartereau, a retired road-surveyor of Angers, has from his office table discovered dozens of proofs of authenticity, and is convinced that he has found in this wonderful site the sources of Gallic writing. M. Butavand, a retired civil engineer, has translated Glozelian by means of Greek roots and the Tifinagh script . . . Each of these gentlemen is almost alone in his opinion, but seldom has any mercy on the rest. Lt.-Col. de St. Hillier in particular is distinguished by the rudeness and severity of his style. He himself wrote his 'Glozelian Grammar' in order to 'establish once for all a sane explanation, free from literary verbiage and based upon a certain and dogmatic foundation' (p. 9).

We are not so sanguine as to expect, of course, that Dr Morlet, M. Salomon Reinach and the little group of persons round them, will ever perceive their mistake. It matters little. In actual fact, whatever may be the verdict of the Law with regard to the forger, the Glozel affair has been shown up so thoroughly that it will never more be a danger to science. One must hope, also, that all the trouble it has created will not have been in vain, and that it will have taught a useful lesson.

Notes and News

THE UR EXCAVATIONS

We reprint in extenso Mr Woolley's communication to *The Times* (25 February 1930) because of its importance, and we wish to acknowledge the permission accorded by the EDITOR of *The Times*, by MR WOOLLEY and by the DIRECTOR of the British Museum.

A fully illustrated account of the lower layers was published in the *Illustrated London News*, 1 March.

'In my last report (published in *The Times* 11 February, p. 13) I described the discoveries in the great shaft which we are sinking in the town site. At that time we had reached a depth of 29 feet, and had found and removed the walls of eight distinct super-imposed buildings. Now we are at 56 feet below the level, which on a conservative estimate we date at 3200 B.C., and outstripping calculation in centuries have to deal with the very beginnings of man's settlement here in the River Valley.

'Below our eighth building there came a change. No more walls of buildings appeared and the soil was little more than a mass of broken pottery. The explanation was soon forthcoming. A brilliantly coloured ring of red and green and pale yellow proved to be a burnt-out kiln of bricks lined with fire-clay, and in the ashes which filled it there were still the clay pots of the last firing. More kilns came to light, covering the whole area in successive levels, basins lined with cement bricks for the kneading of the clay, potters' tools made of baked clay, and pebbles for burnishing the pots. It was a prehistoric factory, and the dense mass of sherds which buried the site was made from the "wasters" discarded by the potter. As the kilns lay four deep the industry must have lasted for a long time, and for so long the regular sequence which marked the upper strata was interrupted; but below the factory level it began again.

'As we went down, the Jemdet Nasr pottery painted in black and red and buff which had characterized the eighth house-level grew scarce, and was supplanted by plain sealing-wax red wares with an admixture

of the black and green pottery familiar to us from Al 'Ubaid. Gradually the proportion of the black and green ware increased, and at last the red vanished and only the Al 'Ubaid wares and plain pottery remained. Then at 42 feet, just when a belt of clean sand made it look as if we were reaching the bottom of all things, graves were found containing plain clay vessels of shapes new to us and, generally, in each grave a cup of that painted Al 'Ubaid ware which, common as the fragments of it are, was represented hitherto by only three fairly complete examples.

'The graves lay thick. Some contained nothing but the body, with others we found simple beads and weapons of stone, but such were rare, and even the clay vessels were not numerous. Whereas the fragments of painted pottery showed a wide range of design, here the complete pots were all of one shape and all decorated in the simplest manner with plain bands of colour. But below these there came more graves, and in them painted vessels of different shapes began to take the place of the plain pots and their decoration grew more and more elaborate.

'The upper graves marked the degeneration of the Al 'Ubaid period, the lower illustrate its zenith. In three of the lower graves we have found objects of a different sort, painted clay figurines of women grotesquely modelled on an archaic convention. Too delicate to be dolls, these queer, slender figures—also one of a painted bird with outspread wings—must be connected with the religion of the race which inhabited Ur before the Flood.

'Already we have reached the levels which mark that disaster, and although one more stratum at least has to be probed, we know from the results of trial shafts sunk in the cemetery area, where the phenomena are precisely similar, that virgin soil cannot be far off. In those shafts the Al 'Ubaid fragments were the last things to be found before we came on clean sand, which we pierced to a depth of some 2 feet below the level of the sea. Thanks to the extraordinarily clear stratification of the soil through which we have dug this season, we have got classified material forming an assured basis for the chronology of Southern Mesopotamia from the time of man's first settlement in the marshes to the close of the Sargonid period in about 2600 B.C.

'For the last ten days work has been going on along the line of the city wall, and though it is too early yet to speak of results, one discovery deserves to be signalized. Two inscribed clay foundation-cones lying against the ruins of the a brick wall led us to start the clearing of the building, and almost at once there came to light in the wall's

thickness the small brick-built foundation-box in which stood still undisturbed the copper figure of the King, bearing on his head the basket of mortar and, before his feet, the stone model brick inscribed with the dedication of the building. It was a temple of Enki the water god, and its restoration by Rim-Sin, King of Larsa, whose statuette we had found, gave its title to the ninth year of his reign'.

In his third report to *The Times* (8 April, p. 15) Mr Woolley writes of the walls of the ancient city, the canals and water-channels traced, the great rampart—26 feet high and from 70 to 90 feet wide—and the temples which have been found.

THE GORGE OF PETRA

Mr George Horsfield, Inspector of Antiquities in Transjordan, has sent us a note on Petra, to which we have added a few impressions of our visit there in 1928. The combined result is printed below :—

Petra, from her geographical situation—between the opposing powers of Syria and Egypt—was subjected to all the cultural influences of the pre-Roman world, attracting, no doubt, by her commerce, a thriving cosmopolitan population, gathered from east and west. The basis of her population consisted of Semitic Nabataeans who had conquered the Edomite lands. These Arabs, coming direct from the desert, were uncivilized, but they soon became rich from their strategic position astride the great trade routes, which met in their neighbourhood. By the second century B.C., their wealth, their ambitions and the increasing weakness of Egypt and Syria in decline, enabled the kings of Petra to extend their kingdom and influence from the Red Sea to the Euphrates.

The extant monuments of this period are mostly tombs, carved from the cliffs of red Nubian sandstone which enclose the city on all sides. They reflect two definite and distinct influences—the one occidental, marked by an Asianic perversion of the classical style ; the other oriental and of the simplest kind, being expressed by a pylon with a plain surface.

The monument (plate I) which most clearly reflects these cultural influences is that which has aptly been called the Khaznah (treasury). It is indeed a treasury of sculpture, combining human, animal and purely decorative motives. It has the quality of great architecture ; and perhaps no other monument in the world has a setting of such majestic

wildness. The style may be described as Asiatic Greek ; it is carved in the living rock, and the execution displays an admirable feeling for an unusual material.

The first sight is most impressive. The only approach to Petra lies along the rocky bottom of a dry water course, between the precipitous sides of a deep gorge, no wider than a dark narrow lane in some oriental town. One rides down this echoing chasm on the back of a half-starved pony whose hesitating gait increases one's feelings of apprehension. Suddenly the gloom lifts and a vision of golden rock breaks upon one, framed between the craggy walls of the canyon and resplendent in the brilliant morning sunlight. This is the Khaznah. It is the first monument the visitor meets on his way to Petra, and it is by far the most impressive. As one gradually emerges into full daylight, one catches sight of other carved porticoes and pediments perched high up on the distant mountain sides. But these, however interesting in themselves, would hardly have attracted so much attention, were it not for their setting of purple and red sandstone, bathed in an atmosphere of radiant sunlight. The fame of Petra is founded largely upon its inaccessibility during the 19th century. To have been there at all was something of an achievement, qualifying one as an explorer in that romantic age. Though now reasonably safe, the journey is not an easy one and it was once both arduous and risky. There is a good motor road from Ma'an, the present terminus of the Hejaz railway, to Elji, distant from Petra about 4 or 5 miles ; and the inhabitants have discovered that it pays better to provide horses for a continual stream of visitors than to hold up a few hardy adventurers. (Their change of opinion dates from the visit of an armoured car section). The romance of Petra lies in the *approach*, and the gem of this is the Khaznah. The arrival is something of an anticlimax for the romantically disposed. In mere rocky grandeur Petra itself is surpassed by Mont Serrat or by the Algerian gorges of Constantine and Meshounesh.

The purpose of the Khaznah and the date when it was made are both of them unknown ; but never again at Petra was such a combination of sculpture and architecture attempted, though the general lines of the design appear again in the Deir and in the Corinthian monument. Hellenistic architecture has disappeared in Palestine and Trans-Jordan except for the monuments at Petra and Arak-el-Amir, and the tombs in the Kedron valley, Jerusalem. The so-called tomb of Absolom at Jerusalem has a close affinity with the circular motive that crowns the Khaznah ; and that of St. James with some of the

square free-standing monuments adorned with cornices and semi-detached columns which are found at the mouth of the Sik. For a comparison with other monuments designed with classical motives, one must go to the rococo of the Renaissance in Spain and Italy, more particularly Spain and Sicily, which have endured a peculiar Semitic influence denied to the rest of Europe. The tomb of Sextius Florentinus is probably Hellenistic in date and certainly in style ; and from the Turkomanya tomb and the Madain Saleh inscriptions we know the curses and penalties employed to ward off those intruders, who seem to have haunted the designers of these elaborate tombs. Sextius Florentinus is apparently an accursed intruder. The Madain Saleh tombs, which date from the 1st century A.D., are vouched for by the inscriptions cut on them ; they give us also the interesting information that they were carved by sculptors bearing Semitic names. These tombs are remarkable for their monotony and for the unfinished crudity of their interiors. The Hellenistic tombs at Petra are notable for the care with which they are set out and cut and for their exquisite interior finish, consisting, in many cases, of a fine tooling of carefully engraved parallel lines at an angle of 45 degrees. Those of the Madain Saleh type, though larger in size, are not more elaborate in detail. We have the date of the one carved to the order of Queen Shaquilath, mother of Rabel II (A.D. 71-106) for her brother Oneishu. Great pains have been taken in the cutting of the façade with its architraves, cornices and crowning steps ; but inside it is as rough as a natural cavern, lacking even the elegance that nature imparts to her work. Amrit (the Ancient Marathus) on the North Syrian coast, whose ruins probably date from the Phoenician period, offers some interesting parallels, connecting the smaller crow-stepped pylon-tombs of Petra and Madain Saleh with the Khaznah. There, out of the living rock is cut a circular pedestal, ornamented with lions; from this rises a cylinder with a rounded top, decorated with a row of stepped battlements, which stand out slightly from the general surface and are supported by square corbels. A similar cylinder lies in the broken pediment of the Khaznah, tricked out with columns, carvings, and cornices, supporting a convex cone, crowned with a capital.

The pylon-tombs are uniform in character, and their use, with little or no variation in detail, continued at Madain Saleh alongside a variant which has borrowed classical details such as columns, cornices and architraves. The tombs at Madain Saleh are dated ; and since exactly the same conditions are found at Petra, it may be concluded

that these plain flat-fronted battlemented pylon-tombs are the most primitive type. It was in use in all periods, and probably preserved an older burial custom ultimately of Mesopotamian origin. From a comparison of the tombs at Petra with those of Madain Saleh it may be inferred that the tradition of Hellenistic architecture was dead by the beginning of the 1st century A.D. In the place of experiments and variations in the classical style, a monotonous variety of the one pylon-form was adopted. The monuments were designed and executed by native artists, who copied one another without venturing to add a new motive or to develop a new idea.

The collapse of the Nabataean kingdom in A.D. 106, the formation of the Roman province of Arabia, with its capital at Bosra in the north and the development of the Red Sea route as the line of communication with the Yemen and India, combined to divert the rich traffic on which Petra's economic life was based. The glory of Petra passed with her declining trade, and she was left with nothing but the memory of her former greatness. Hidden in her mountain fastness and surrounded by the desert, she was no longer the emporium of the East, the capital of kings ruling from the Red Sea to the Euphrates, but merely an obscure and half forgotten provincial town.

Though temporarily revived under a Byzantine administrative measure to become the capital of Palestina Tertia, the seat of a bishopric and a place of exile, Petra eventually sank into an utter oblivion from which she was only rescued again by the romantic curiosity of the explorers of the last century.

THE DESICCATION OF AFRICA

Mr H. S. W. EDWARDES writes :—

'The controversy over desiccation in Africa is to keep us interested for a long time yet. Mr E. W. Bovill's article in ANTIQUITY (III, 414-23) will I hope draw a reply.

'The question is often confused by a failure to distinguish between geological time and historical times.

'Fluctuations may have occurred in the remote period when ice covered a large part of Europe, but is there evidence of any temporary improvement in historical times, say since 5000 B.C.? Throughout the Sahara, even in the intensely desiccated area east of Kufara, Neolithic implements are found. The owners of these implements had no camels, and almost certainly no transport animals of any kind.

They must have found water at intervals of 30 miles or so, and vegetation capable of sustaining a fauna for them to hunt, or of providing them with grain. The flints found by Col. de Lancey Forth, (*Geographical Journal*, Jan. 1930), were more than 100 miles from water and in absolute desert. I don't know how these compare in age with the Nile Valley flints, but their age merely affects the rate of desiccation. (That any action of these folk themselves could have affected the matter is incredible).

'In Roman times, when Cyrenaica carried a large population, the rainfall may already have fallen close to the line below which corn cannot be grown. A small further fall would then throw great areas out of cultivation. One would like to know more about the Roman bridges. Are the rivers they cross still perennial? The Sokoto river, above the city, is bigger than the Thames for two months in the year, and bone dry for six months. I have seen El Djem and its surrounding desert. Is there evidence of the vast irrigation system that must have existed if such a city was to be fed with corn not raised by rainfall? Man may have helped the desert to advance by failing to withstand it, by allowing the fatal goat to destroy the protecting bush, and by reckless burning, but his sins of omission and commission can be but minor factors. The evidence of the Neolithic finds throughout the Sahara seems to me conclusive that true desert conditions have only come about within the past 10,000 years or so, and they have spread steadily and continuously down to the present day'.

DIGGING STICKS

Among the various ethnological specimens which have been found in the Sudan recently are stone hammers for beating copper and stone or clay rings. These rings are too large for spinning whorls and expert opinion varies considerably as to their use. Some of the uses suggested are mace heads, balancing weights for throwing spears, weights for nets, weights for digging sticks, etc.

The primitive bone-headed spear in use until recently on the Sobat river had no balance weight on the end nor can I trace any net which was weighted. The casting net or basket-trap was generally in use among primitive peoples.

The accompanying plate, from a photograph taken by me, is both a link with the vanished past and an illustration how primitive man obtains great results with the least possible bodily fatigue. It shows Dinkas using their digging sticks to break up the ground after burning

off the grass and weeds. These sticks vary in length from ten to fourteen feet, and are about the thickness of a man's wrist at the sharpened end, which is hardened in the fire.

These digging sticks are not artificially weighted and are different implements from the Arab dibber. The long sticks take the place of a plough or heavy hoe. The Arab dibber is a curved stick which is pushed into the soft mud or earth (after flood, rain or irrigation) by the big toe. It is then given a circular motion and thus leaves a conical hole into which the sower drops the grain and closes the hole with a foot. The dibber is not weighted and is used for the planting of cotton, dura, etc. A. E. ROBINSON.

PRIMITIVE HUTS*

In most popular text books on ' Ancient Britain ' or the prehistoric period it is assumed that thatched huts had central roof poles. The writer suggests that the people who possessed sufficient intelligence to build corbelled stone huts or mud huts like those of the Nile dwellers without arches or supports could have employed methods of construction, thatching and so forth similar to those now used in Africa.

The Sudan *tukl* (circular hut) has no central pole. The conical top or roof consists of a very light framework of thin branches and it varies in shape from a perfect cone to that of an open umbrella. This framework is made of radial poles fixed into a circumferential base of flexible withies ; and it is thatched and complete before it is lifted on to the walls of mud, brick, dry stone (without mortar), or straw, to which it is affixed. If the circular walls are of straw it is usual to drive four stout forked poles into the ground so as to take the weight of the roof. In most of these huts the cooking is done over a fire in the centre of the hut and the smoke keeps out mosquitoes, etc.

I suggest that the huts discovered by Mr H. S. Toms (see A. Hadrian Allcroft, *Earthwork of England*, p. 253) may be huts of the *tukl* type. I saw huts made entirely of reeds in the treeless marshes near the mouth of the Danube forty years ago and although of a similar type to the *tukl* they were not as substantial nor as well finished as the African product. Some of the restorations which I have seen recently appear to me impracticable, since the structures (unless made of heavy timber) would not bear the weight of a thatcher. A. E. ROBINSON.

* We are indebted to Mr G. N. Morhig, of Khartoum, for his good offices in connexion with plate III, which is reproduced from one of Mr Robinson's photographs.

A POLISHED AXE FROM SOUTH AFRICA

The finding by Professor Drennan in South Africa of a polished stone axe associated with kitchen-midden material (*Cape Times*, 13 February 1930) is a matter of some interest to prehistorians. Very few polished axes have ever been found in South Africa, and this is the first time that an association with a definite culture has been demonstrated. It is true that rare instances of ground edges occur in Wilton industries, but no true polished axes have been found. The new axe comes from a small rock-shelter at Witsands, beyond Kommetje, where excavations have only yielded kitchen-midden material. The ground surface is greater on one side of the tool than on the other, extending two inches from the edge on the one side and only half an inch on the other. In this respect it resembles the other polished axes found in the Union of South Africa and also certain examples from Australia ; on the other hand the well-known specimen from Battlefields, Southern Rhodesia, is polished all over and recalls rather the type found in the Congo. Professor Drennan considers that the newly found axe was undoubtedly hafted, as a polished groove exists at the butt end of the tool round which a withy could be bent and then fixed with gum and thong. This groove appears to have been made by pecking and then rubbing. The material from which the tool is made is a local rock.

Professor Drennan has also described in the *Journal of the Medical Association of South Africa* for November 1929 an important find of human bones in a sand quarry on the Cape Flats near Cape Town. The industries found comprise kitchen-midden and Still Bay types. Some of the bones are of the ordinary Bushman type and with these Professor Drennan associates the kitchen-midden tools. But a skull and a femur belong to quite a different and more Australian-like individual, and perhaps may be associated with the Still Bay industry. Much controversy has raged as to the former existence in South Africa of an Australian-like race. Rhodesian man has been cited as an example of this proto-Australian type and recent researches by Mr Zieve at Cape Town seem to support this view, as perhaps also do these newly-found human remains. The cranium is almost complete and the dimensions are identical with those obtained by Hrdlicka from an examination of 190 South Australian skulls, though the angular measurements are by no means identical and the face is in several respects decidedly negroid. The prominence of the brow ridges however is a special feature of the skull which distinguishes it from Bushman or Bantu specimens.

Calculations from the size of the femur suggest that the individual may have been about $5\frac{1}{2}$ feet high. The former wide extension of the Australian and Melanesian races has been lately stressed by Professor Paul Rivet. Examples of the skeletons of both races seem to occur in South India, and if this is the case it is quite likely that South Africa was not unknown to these peoples. M. C. BURKITT.

BREWING IN ANCIENT TIMES

The Gesellschaft für die Geschichte und Bibliographie des Brauwesens, E.V. (Institut für Gärungsgewerbe, Berlin) has for its chief object the publication of monographs on the history of brewing. One of the first of these is by Dr Huber, entitled *Bier und Bierbereitung bei den Volkern der Urzeit*: I. Babylonien und Ägypten, 1926. In this Dr Huber discusses beer and brewing by the peoples of Babylon and Egypt. His evidence, which is based on the rather scanty records obtained from seal-cylinders and monuments in tombs of the period, indicates that agriculture was practised as far back as 6000 B.C.* Now agriculture has always been associated with the baking of bread and the brewing of beer, and there is abundant evidence in fact that both these occupations were practised in these early times. But malted barley is the starting-point for the brewing of beer, just as wheat is for bread, and here again we have evidence of the two crops cultivated at the same time and as of equal importance. Of course it must not be assumed that barley only was used for beer, since 'mixed' beers, which were made from a 'grist' containing barley with 50 to 70 per cent. of wheat, were also brewed.

The importance of beer in Babylon and Egypt has probably not hitherto been fully recognized and some of Dr Huber's conclusions are of great interest. Thus we learn from Babylonian pay-lists and inscriptions that beer was drunk in large quantities, but that rations were fixed according to the class of employment. Officials were provided for at the liberal daily rate of about 8 or 9 pints, brain workers and harem women were allowed 5 pints, though in the latter case the beer was a sweeter type, while the labourer had less than 2 pints of a very weak beer.

By the time of Hammurabi the importance of beer had increased to such an extent as to render necessary strict laws controlling its

* All absolute dates before 2000 B.C., in any part of the world, are more or less conjectural.—EDITOR.

preparation, price and sale. Daily offerings made to the deity Ishtar—according to the ancient belief that gods required the same form of sustenance as man—contained barley and two pints of good strong beer. In medicine also, beer was frequently used both as a cure and as a means of diluting the less pleasant constituents, and such forms of medicine no doubt attained a measure of popularity, though the records are silent as to whether they were effective.

In Egyptian times the importance of beer as a national drink was supreme. It was drunk by people of all ages and classes, and even workmen were content to receive it as a part-wage. The use of wine was almost entirely supplanted, for the art of brewing had passed largely into the hands of the housewife. Needless to say the beers must have varied considerably in quality, and Dr Huber's work leads us to believe that the beer of the time was a turbid, easily-infected liquid prepared under the most primitive conditions. Hops were not used, and the beer had to be drunk through a long tube in order to avoid the sediment. Perhaps the Egyptians considered there was ' no such thing as bad beer ', though that the dangers of excess were realized is shown by the fact that the young were warned that ' when thou drinkest till the demon seizes thy heart . . . the next day wilt thou be unable to work'. Dr Huber's most remarkable achievement, however, is his description of the reliefs from ten Egyptian tombs, which, when pieced together, give 23 scenes representing the complete brewing process of the time.

Many Egyptian words connected with brewing may be traced from the corresponding Sumerian–Babylonian equivalents and Dr Huber's work therefore also throws an interesting light on the relative ages of these two civilizations which, as we have seen, are intimately connected with that of Civilization itself. JULIUS GRANT.

SURGERY WITH FLINT

The notes which follow deal with some surgical operations performed about thirty years ago by the Loucheux Indians* of the Yukon Territory of Canada. These Indians had not at that time been spoiled by too close a contact with white men, and were in what might be described in a general way as a 'neolithic' stage of culture; consequently this record of their skill in surgery is of some interest for

* This name is believed to cover the Tukkuthkutchin and the Tatlitkutchin sections of the Kutchin tribe, one of the Athapascan language-group.

the sake of the comparison that may be drawn between them and those prehistoric peoples who are known to have practised quite serious operations, such as trepanning, with success.

The facts were communicated to the writer by Mr George M. Mitchell, of Quebec ; and he must be considered an authority seeing that he lived with one of these tribes for more than two years, was raised to the rank of chief, and also obtained the closest insight into their methods by personal experience.

In February 1899, Mr Mitchell found himself near the summit of the Ogilvie Range at approximately lat. 64°45′ N, long. 134°45′ W. A tree which he had occasion to fell slipped backwards off its stump instead of falling clear, and the butt struck him on the centre of the left knee, breaking the patella. This injury made him quite helpless, as, when the patella is broken, the limb cannot be straightened by muscular power. The nearest civilized surgeon, being more than a thousand miles distant, Mr Mitchell had no choice but to submit himself to the local 'savage' practitioners. These in his case were the women of the tribe to which the men of his party belonged ; surgery among these people being a function of the women and not of the men.

His retainers accordingly conveyed him to their camp on a toboggan drawn by dogs, after strapping his leg to a long wooden splint, and though they covered the distance of sixty miles in less than eleven hours the leg was naturally very much swollen when they arrived. The most expert of the women, assisted by a younger girl who happened to be the chief's wife, immediately gave the leg a careful examination, and, as she decided that an operation should be performed, preparations were begun without delay, though it was then the middle of the night.

As a preliminary they fixed the leg rigidly into a pair of wooden splints that extended from the thigh to the ankle and were shaped carefully to fit : these were made very quickly and skilfully by some of the men. Then came the knife—a flake of flint an inch and a half in breadth and having a natural cutting edge, that was struck on the spot with the back of an axe from a rough lump of flint that was kept at hand for fire-making. Mr Mitchell later asked the chief why they had used flint instead of the very good Sheffield knives which they possessed in abundance, and was told that a fresh flint knife was used because it was clean, whereas a steel blade would have been dirty. But although this answer might be taken to suggest that the Indians had some idea

of the importance of avoiding infection Mr Mitchell does not remember that they made any attempt to wash the leg, or even their own hands, before they began to operate.

When everything was ready, and a crowd of Indians had packed themselves into the lodge to see the spectacle, the chief's wife proceeded to the actual operation under the careful instruction of the older woman ; the latter, though she had much more knowledge and experience, was handicapped by possessing a tender heart, whereas the former thoroughly enjoyed inflicting pain. She made three cuts : one about three inches long down the inner side of the knee, one crosswise below the knee to the outer side of the leg, and a third, corresponding to the first, up the outer side ; and then, seizing the flap of skin that was thus released on three sides, she flayed it upwards so as to expose the patella. This proved to have been split horizontally, the two fragments being separated from one another in a vertical direction.

Now it is clear that their preliminary examination had shown the two women that this was what they would find, for in the meantime the men had been busy preparing a number of small pins of caribou bone. These the women inserted into the tissue just above the upper fragment and just below the lower one, leaving their heads projecting slightly, and then, taking caribou sinew that had been pulled out to about twice the thickness of the coarsest sewing thread, they wound it backwards and forwards round the pins so as to draw the two fragments of the patella together. When the fragments were firmly united in this way they replaced the flap of skin and bound it into place with thongs, without sewing up the wound.

During the operation Mr Mitchell became unconscious several times, and he remembers that whenever he came to he found small bundles of deerskin containing hot sand or live coals in the palms of his hands. He thinks that these were only applied as a last resource, when the operators feared that without them he would not regain consciousness at all. He thinks that the whole operation may have taken two or three hours to complete.

Subsequent treatment consisted of the application every three or four days, for about a month, of a hot poultice made of various herbs and the inner bark of a willow ; and whatever the qualities of this compound may have been the wound healed in six or seven weeks without any infection. The bone pins worked their way out through the skin in the course of the ensuing three years, and the sinew appears to have been absorbed.

Mr Mitchell remained on his back for about three months, and after a further six or eight months was able to undertake the long return journey down the Peel and up the Mackenzie rivers. During the whole of this time he continued to wear his splints and to use a long two-handed pole, the Indian substitute for a crutch. On his return to civilization the doctors found that the patella was perfectly knit, and though a long period of treatment was required to restore the withered muscles and loosen the stiff joint the leg was eventually brought back to a perfectly sound condition.

However, as this operation is being considered as an example of surgery as practised by a primitive people, it would be an exaggeration to give the Indians full credit for the complete recovery that Mr Mitchell eventually made. His own opinion is that an Indian who had suffered the same injury would have worn his splints and used a crutch for the rest of his life, as the risk of breaking the leg a second time would have been too great to allow him to dispense with them, and the Indians have no knowledge of massage or any other artificial means of restoring suppleness and strength. But even so the mere fact that such an operation could have been performed successfully, and without killing the patient through shock, tetanus, gangrene, or whatnot, is sufficiently remarkable in itself when all the circumstances are taken into account.

Another interesting case that came under Mr Mitchell's own observation was that of a young Indian who was shot accidentally during a bear-hunt—a rifle bullet entered his chest from the front, just above the heart, was deflected upwards, and came to rest in the muscles of the shoulder between the back of the neck and the scapula. The women who examined him were puzzled at finding no wound of exit, but proceeded to search for the bullet in the manner of civilized surgeons with flexible probes, made out of pliant shoots of willow, which they inserted at the wound of entry. Having eventually located the bullet in this way they removed it through an incision in the patient's back. After his recovery the patient seemed to be none the worse, and returned to his ordinary avocations.

Perhaps the most remarkable point in this case was that the women were careful to leave the probes sticking in the wound after they had extracted the bullet, and only withdrew them by degrees as healing progressed outwards from within. They were evidently quite aware of the danger of allowing a deep gunshot wound to heal superficially while any danger of infection at the bottom remained.

It appears, however, that in spite of their undoubted skill these

Indians consistently refused to carry out any kind of amputation. Mr Mitchell noted this on several occasions, and quoted the following case as a rather extreme example. An Indian was brought in whose arm had been clawed by a bear; most of the flesh had been torn off the upper arm so that a good part of the humerus was exposed, and bleeding had only been stopped with great difficulty. The chief asked Mr Mitchell's advice as to what should be done, and, as it was clear that the arm could never be serviceable again, he suggested to the chief to have it cut off at the shoulder—this being an operation that was well within the powers of the skilled women of the tribe. However the chief refused point-blank, and simply had dressings applied; and as a result the man, though he survived, was condemned to a miserable existence with a withered limb permanently bound across his chest. Mr Mitchell never managed to satisfy himself as to the real reason for this dislike of amputation, but believes that some religious scruple may have been involved.

ANGUS GRAHAM.

EXCAVATIONS, 1930

When this number appears the digging season will have opened in Great Britain. The event of the year will be the excavation of Verulamium, the Roman mother-city of St. Albans. The site has been kept free from modern buildings through the public spirit and foresight of the owners. The complete excavation of the southern portion only will be a matter of years. The work will be directed for the Society of Antiquaries of London by Dr Mortimer Wheeler, M.C., Keeper of the London Museum. We hope that it will be possible, if not this year perhaps later on, to make a thorough examination of the pre-Roman defences, which are constructed on a colossal scale. The excavations necessitated outside Colchester by the cutting of a wide by-pass road should yield most important comparative evidence. Indeed the two sites have much in common.

Wessex is, as usual, well to the fore, and although detailed information is not yet available, we know of at least three important new undertakings in addition to such hardy annuals as the Meare lake village and Windmill Hill.

Recent Events*

The Editor is not always able to verify information taken from the daily press and other sources and cannot therefore assume responsibility for it.

An International Congress of Anthropology and Archaeology is to be held in Portugal between 21 and 30 September next. It will begin at Coimbre, continue at Oporto, and conclude at Lisbon. Participation in the Congress is open to members of L'Institut International d'Anthropologie (subscription 40 francs a year, which includes the *Revue d'Anthropologie*) and to others who send 40 francs to the Secretary of the Institute (15 rue de l'Ecole de Medicine, Paris) from whom a detailed programme of the Congress may be obtained. The Congress will be divided into a number of sections, and everyone, including those without any specialized knowledge, will find much to interest them in the various papers. Certain reductions will be made in the fares on railways and boats for the benefit of those who enroll themselves before 1 August.

Messrs Methuen are issuing a series of County Archaeologies and the first volumes, on Middlesex, Berkshire and Kent, will be ready shortly. Each volume will contain a concise account of the archaeological material, from the Old Stone Age to the Norman Conquest, together with a detailed gazetteer containing full references, a list of museums, a bibliography, and a folding map. The General Editor of the series is Mr T. D. Kendrick, of the British Museum. The series should provide a popular and at the same time authoritative set of handbooks and we wish it every success.

* In this section are published such notes about forthcoming events as are received. The special section on Forthcoming Excavations had to be discontinued on account of the difficulty of obtaining the necessary information in time for publication. We regret this as we think that excavators themselves might have profited, especially in cases where an appeal for public support is made.

NOTES AND NEWS

One of our readers informs us for the benefit of those interested in the Middle East that a full conspectus of recent work out there is to be found in the *Bulletin of Associates of Fine Arts*, Yale University, February 1930.

The ' Runic inscription ' which we commented upon in our last number (pp. 108–9) was also exposed by the *Glasgow Herald*, 12 November 1929. We regret that, not being aware of this at the time, we made no reference to it.

As the result of excavations carried out last April on the camp on Holmbury Hill, Surrey, Mr S. E. Winbolt reports evidence of 'flint-working people, neolithic or later: flint flakes and implements, and (possibly) sling pebbles. Early Iron Age: (possibly) sling pebbles, many beehive querns, gritted red pottery, La Tène III soapy black ware. Roman Age: a dark grey rim and possibly the sling pebbles'. As he reads the evidence, 'the defences of the camp were thrown up, possibly on a neolithic site, in the Early Iron Age, and continued to be inhabited in the Roman era'. (*The Times*, 29 April 1930).

We have received from Miss Liddell a copy of a type-written report on excavations carried out under her direction at North Warnborough, Hants., during March and April of last year. It is beautifully illustrated and reflects the utmost credit on all concerned. The site contains the remains of a Roman villa and we are glad to hear that work has been continued this year with very satisfactory results. Readers of ANTIQUITY will recall Miss Liddell's paper in a recent number (September 1929, pp. 283–91) on the ornamentation of prehistoric pottery, and will hope when the present work is complete that she will find a prehistoric site to excavate.

A new Roman fort has been discovered at Newbrough, near Hexham, on Hadrian's Wall. The church stands inside, and part of the churchyard wall is on the wall of the fort. Trial excavations have been made by Mr F. G. Simpson. (*Evening World*, Newcastle on Tyne, 5 and 6 March).

A similar but less happy discovery has also been announced by the press. An air-photograph published by the *Daily Express* (Scotch edition, 26 March) has the following title :—' Faint traces of the Roman camp at St. Abb's Head, near Berwick, picked out by the eagle eye of the Aerial Camera'. Every word of this is wrong. The traces are not faint but quite plainly marked ; it is not a camp, nor if it were would it be likely to be Roman since it is almost mathematically circular; it is not at St. Abb's Head but 5 miles to the southwest; and it was not discovered by either camera or pilot, since it is marked on the Ordnance Map. Actually the circular bank is of quite modern origin, and was probably thrown up round a clump of trees. The photograph was submitted to us soon after it was taken, and the main facts were at once pointed out. It is therefore unlikely that ' Scotland will learn a new and unsuspected story of her national history' from this particular site, though air-photography in the Vale of Strathmore for example might well make an important contribution to knowledge. But the 'eagle eye' must beware of the goose's quill, lest it be put out thereby.

Another gaucherie was published in *The Sphere* (8 March), where 'old mediaeval tiles emblazoned with an embryonic (*sic*) English lion' are described as 'relics of Rome'. The picture shows plainly that they are medieval, but does not throw any light on the true nature of the lion. It should be added that the tiles were found at Great Berkhamsted Castle in Herts., and that the site of this name associated with William the Conqueror's march on London is more likely to be Little Berkhamsted, near Hertford.

It is good news to hear that Sir Philip Sassoon is promoting air-photography of ancient sites in his new command, No. 601 (City of London) Squadron, Auxiliary Air Force, and that the subject is arousing interest. (*Daily News*, 28 February).

Interesting Roman remains have been found in King's Meadow, Carlisle, a second century site, among the finds being a very fine pair of trumpet brooches on a chain. Mr R. G. Collingwood, F.S.A., has prepared a preliminary report. (*The Times*, 15 March, p. 9).

An interesting suggestion to account for the invention of mud-brick building has been put forward by Mr S. R. K. Glanville, of the British Museum. He says :—

' From a very early time it must have struck the Egyptians that the large, irregular blocks of dry Nile mud, which were left as the retreating river's banks hardened and cracked in the strong sun, could not only be used themselves as a serviceable building material, but could also be imitated by drying lumps of wet mud—regardless of the state of the river. Thus, doubtless, bricks came to be invented. At first they seem to have been used merely as subsidiary to the sandy or rocky walls of scooped-out graves and hut-circles. But, as the shape of the brick became more definite, its possibilities were appreciated, and buildings constructed entirely of sun-dried mud-bricks became the rule—and have remained so ever since. (*Illus. London News*, 8 March).

The discovery of a very primitive skull at Choukoutien, near Peking, on 2 December 1929 is of prime importance. Prof. G. Elliot Smith, writing to *The Times*, 16 April, speaks of it as 'the most impressive, and probably the most important, contribution to our knowledge of early Pleistocene Man that has yet been made'. The skull was embedded in very hard stone (travertine) but Dr Davidson Black, to whom the find is due, has succeeded in removing it so that 'there is revealed for the first time the whole brain-case of an early Pleistocene Man'. Prof. Elliot Smith points out certain misconceptions which are now cleared up by the evidence which the skull presents. An illustration of the right side of the brain-case is in the same issue of *The Times*, and illustrations of the skull, from several points of view, appeared in *Illus. London News*, 3 May. Illustrated memoirs published by the Geological Society of China are noticed in *The Times*, 5 May, p. 9.

The excavations of Sir Charles Marston and Professor Garstang at Jericho have aroused considerable interest and we look forward to the publication of their results. (*Daily Telegraph*, 10–11 February).

Seven skeletons have been found near Boulogne in a tomb formerly buried beneath sand-dunes and recently denuded by the wind. (*Daily Mail*, 17 February).

ANTIQUITY

An account of recent archaeological work in India by Mr H. Hargreaves, Officiating Director General of Archaeology, has been published in *The Times*, 18 February. We understand that the long-awaited account of Mohenjo-Daro will be issued shortly.

A hoard of gold and inlaid jewelry has been found on a site at Sirkap near Taxila in India. (*The Times*, 25 January).

The mummified remains of a rhinoceros have been found in an ozokerite mine at Starunia in Eastern Galicia. (*Nottingham Guardian*, 16 January ; *Ill. London News*, 18 January).

Steps are being taken for the uncovering under expert supervision of certain portions of the Roman town of Isca Dumnoniorum (Exeter), where old buildings are to be cleared away.

St. Isaac's Cathedral, Leningrad, is to be transformed into an anti-religious museum. (*Manchester Guardian Weekly*, 17 January).

An appeal for £5567 has been issued by the officers of the British School of Archaeology in Iraq (Gertrude Bell Memorial). Out of the £20,000 required to endow the School, £6000 (bequeathed by Gertrude Bell herself) is being held in trust by the Trustees of the British Museum, and no less than £8463 has been raised by public subscription. Those who attended the crowded inaugural meeting in the Central Hall at Westminster, presided over by Sir Percy Cox, Chairman of the School, will not be surprised at the success of the appeal. We hope the remaining sum will soon be forthcoming. Cheques should be made payable to the British School of Archaeology in Iraq and sent to the Honorary Secretary (Sir Edgar Bonham-Carter), 17 Radnor Place, London, W.2. We feel sure that the School has a great future.

Detailed particulars have been published of Dr Selim Hassan's discovery of the tomb of Ra-Wer (Fifth Dynasty), who among other titles enjoyed that of High Priest of the Goddesses of Egypt. The tomb was found near the Sphinx and is said to be the largest private tomb of the old kingdom hitherto known. (*Morning Post*, 17 February).

The fifth of the British Museum's expeditions to British Honduras for the excavation of Maya antiquities is now at work under the direction of Captain E. L. Gruning. The investigation of the site at Pusilha will be continued. (*The Times*, 22 January 1930, p. 17).

An exhibition of the Stone Age skulls discovered by the East African Archaeological Expedition in Kenya under the direction of Mr L. S. B. Leakey was held at the Royal College of Surgeons in January. One skeleton, though of a comparatively late period, is specially interesting. It was found at the lowest part of a great burial mound, covered with parts of other skeletons (presumably human sacrifices at the burial of a chief), fine stone pots and mortars, bowls and tools. Faience and agate beads were also found, and are taken to indicate that the Stone Age people were in touch with traders from the civilizations of the time. (*The Times*, 22 January 1930, p. 9).

An interesting account of the results of the excavations made at the early Christian church of San Sabastian in the Appian Way was communicated to *The Times*, 20 January 1930 (p. 11). The work was begun by the Pontifical Archaeological Commission in 1915 and has been continued, except for suspension during the War, ever since. The foundations show that the building was constructed over some Roman houses dating from the 2nd century A.D.

A relic of Buddha, which is regarded as authentic by the Southern Circle of the Indian Archaeological Survey, is reported in *The Times*, 29 January 1930, p. 13.

A lecture on the excavations at Caistor, next Norwich, by Prof. Donald Atkinson, F.S.A., in which details of the temples found on the site were given, is reported at some length in *The Times*, 22 February, p. 9.

Excavation on the early Trojan site of Thermi has been resumed by the British School at Athens under the direction of Miss Winifred Lamb. (*The Times*, 13 March, p. 12).

Reports on the excavations at Herculaneum, which have been in progress since May 1927, are printed in *The Times*, 23 January, p. 11, and 25 April, p. 11. The excavators aim to preserve the original construction of the houses, with their special architecture and styles of decoration.

The report of the Associazione Nazionale per Aquileia, giving the first results of the excavations in progress at Aquileia has been published in *The Times*, 1 February, p. 11.

A cemetery dating from the 7th or 8th centuries A.D. has been found near Oerligen, in Canton Zurich. Over 50 tombs were examined, 38 of them having grave goods. (*The Times*, 20 February, p. 13).

Excavations carried out last year by Professor Walter Schmid of Graz, Austria, at S. Margarethen, have established the fact that this village was the ancient Noreia, the capital of the East Alpine kingdom of Noricum. The town is first mentioned in 113 B.C., when the great battle between the Romans on the one hand and the Cimbri and Teutones on the other took place near it. The kings of Noricum were allies of the Suabian king Ariovistus and of Julius Caesar. After the conquest of the country by the Romans in 16 B.C., a new capital, Virunum (Zollfelde near Klagenfurt), was founded, and the name of Noreia was transferred to an adjacent Roman posting-station. Today the memory of it survives only in the legend of a buried city at Horfeld.

The archaeological discoveries consist of houses of one or two rooms each, one being claimed as that of the king. The pottery belongs to the late Celtic period (2nd and 1st centuries B.C.), and to the beginning of the Roman Empire. Close by are the 'gold and silver pits' mentioned by Strabo, and remains of the iron-mines for which Noreia was famous.

Reviews

PAINTING IN ISLAM : a Study of the Place of Pictorial Art in Muslim Culture. *By* Sir Thomas W. Arnold. *Oxford : Clarendon Press.* 1928. *pp.* xviii, 159, *with* 65 *plates of which* 8 *are in colour.* 84*s.*

This book is one of the most successful examples of printing and production that have ever appeared from the Clarendon Press, where meticulous care and artistic taste have long been happily combined. The reproduction of the plates, both in colour and in half-tone, is remarkably good, but the typography of the letterpress pages is equally a delight to the eye. As one comes to study the text of the volume, the first impression is of immense learning, increased by the elaborately accented Arabic names—transliterated according to the latest practice of scholars—and by the plentiful references to original sources in the footnotes. But on further acquaintance, one finds that this apparently ponderous text is very readable, and is illuminated by frequent flashes of real humour, as in the comparisons between Christian and Muslim laxity in matters of religion, or in the amusing legends of King Solomon and the Queen of Sheba. Moreover, it contains a number of charming translations from the Arabic, many of them in blank verse, which are not ascribed to any translator by name and are therefore presumably the work of Sir Thomas Arnold himself.

In his preface he explains that ' the present work makes no claim to be a history of Muslim painters ' and that ' the purpose of the book is rather to indicate the place of painting in the Islamic world, both in relation to those theological circles which condemned the practice of it, and to those persons who, disregarding the prohibitions of religion, consulted their own tastes in encouraging it '. The author has restricted himself to these self-appointed limits, but if his work should reach a second edition there must be many students who would welcome the inclusion of a bibliography, and it might be desirable to give dimensions, in inches or centimetres, of the pictures reproduced, as is usually done in the case of larger European paintings. This slight addition could easily be inserted in the ' Notes on the Illustrations '.

There is very little information about technique in the book, such as one finds in M. Saladin's *Manuel d'art musulman* (p. 6, etc.); but on the other hand there is a good deal of new matter relating to individual artists, which is too often lacking in works of this kind. The author's researches have not enabled him to give us a Muslim counterpart of Vasari (though apparently Maqrīzī in the fifteenth century did compile such a collection), but throw a good deal of light on the Muslim painters' state of mind, social position, methods of work, and remuneration.

It is well-known that the representation of human figures and all other living forms was condemned in the Islamic world, not, as is commonly supposed, by any specific reference in the Koran, but by passages in those ' Traditions of the Prophet ' which have always been considered to be divinely inspired, and carry almost as much weight as the Koran itself. The author suggests that hostility to figure-representation was of Jewish origin, and that Muhammad himself was less intolerant, for did he not allow his child-wife Ā'ishah (aged nine) to continue playing with her dolls in his tent ?

Iconoclasm was a later development. But although the mosque remained, in all countries, unviolated by figure paintings, the prohibition came to be disregarded in the secular life of the court, despite the ceaseless hostility of the theologians. Yet the Koran itself, while it inspired that wonderful ornamental calligraphy which is perhaps the chief glory of Muslim art, was never illustrated with figure-painting. (Oddly enough, the ' Arabian Nights ', an ideal subject for artists of the Persian school, seems never to have attracted them). Muhammad himself is represented in no surviving pictures earlier than about 1300, and from the 16th century onwards his face is veiled.

Painting in Islam, up to 1600 or so, was mainly confined to miniatures in manuscripts. There are famous wall-paintings at Quṣayr 'Amra (8th century) in Moab, and at Sāmarrā (9th century) in Mesopotamia, but these are exceptional and both were almost certainly executed by Christian artists. Highly erotic paintings on the walls of royal baths were favoured in Persia at a later period, and the learned author contrives to get a good deal of amusement out of describing them. But very few examples of painting of any sort are earlier than the 15th century. Nearly all were miniatures, and the oldest important example of this work, the Maqāmāt of Ḥarīri (13th century), is ascribed to Christian artists. The first people to encourage painters to transgress the Islamic prohibitions were the same powerful rulers of the eastern Muslim states who broke other rules relating to wine, eunuchs, and tomb-building. At first they utilized the services of artists from the large Christian communities still surviving under Muslim rule, and to this source we owe the round haloes used in early pictures. Later, these were superseded by characteristic ' flame-haloes ' imported by Chinese artists, together with dragons, and many animal and bird forms from far Cathay. Other elements in early Islamic painting were contributed by the revived Greek school at Ḥarrān in Mesopotamia, the Manichaeans in Khurasan, and the Sasanian remains in Persia. Large numbers of Islamic paintings of later date are preserved in public and private collections, but far more have perished. The paper on which they were executed in delicate colours is easily damaged and destroyed, the great Muslim cities where they were most numerous suffered much by war even after the terrible Mongol invasions of the 13th century, the ladies of the *ḥarīm* (according to the author) decorated them with dirty finger-marks, and iconoclasts burned them in quantities. Most of the surviving examples are Persian and Indian : the western parts of the Muslim world (Spain, North Africa, Egypt, Syria, Turkey) seem to have followed the orthodox prohibition.

The subject-matter of the paintings provides the author with some of his most interesting chapters, and includes several national epics, full of fighting and bloodshed ; a quasi-scientific book by Qazwīnī on the ' Marvels of Creation ' ; and a whole galaxy of Muslim ' prophets ' and saints. Among these are several of the chief figures of Christianity : Jesus, Abraham, Joseph and Solomon among them. Some of the legends of Jesus are recorded (though often in a different version) in the New Testament. Others are unknown in the Christian world. The sacrifice of Isaac is the subject of a highly realistic picture (xxxi), while the Queen of Sheba's appearance at Solomon's court brings us almost into the realm of comic opera. Jonah and the whale are only a degree less amusing ; and next in this strange gallery comes Alexander the Great, whose adventures on the ' Island of Women ' (xxxvii, 6) Sir Thomas Arnold discreetly ignores. Last of all we meet a variety of horned jinns, dervishes, and mystics, to say nothing of Burāq, the horse with a woman's head on which Muhammad rode from Mecca to Jerusalem. She gets a whole chapter to herself.

The author makes several important statements in regard to Islamic painting in

general. He points out in one place (p. 37) that Muhammadan literature contains no attempt ' to work out any independent system of aesthetics or arrive at any appreciation of art for its own sake ', a pertinent and penetrating observation. His eighth chapter explains the almost complete absence of any effort at portraiture in painting, and the ninth shows that, with a few exceptions due to alien influence, emotion is never expressed. Thus the painting of Islam, in spite of all the charm of colour and composition and pattern that it displays, lacks the human and spiritual interest that counts for so much in Western art

MARTIN S. BRIGGS.

THE MOST ANCIENT EAST: THE ORIENTAL PRELUDE TO EUROPEAN PREHISTORY. *By* PROF. V. GORDON CHILDE. *Kegan Paul.* 1928. *pp.* 272. 15*s.*

Prof. Childe's book was produced very quickly, and bears some signs of haste. But it is nevertheless a *tour de force*. It is a very useful book, and will remain so for some little while, till the author brings out a new edition, considering the latest developments of excavation and the latest critical work on the subject. It is a book that might very well be brought up to date from time to time. It gives the general reader, as well as the archaeologist, a competent conspectus of recent archaeological discovery, and will be specially useful to the European archaeologists, who must now more and more acquaint themselves with the results of Oriental discovery. The archaeological world is growing smaller every day, and in all directions spheres of work and thought previously independent are impinging on one another.

Prof. Childe summarizes in turn the Egyptian predynastic age in the light of recent discovery, from the Badarian period to the rise of the dynastic age in Egypt; the two ' prediluvian ' cultures of Susa and their connexions; the Sumerian age on which so much fresh light has been thrown by Mr Woolley and Prof. Langdon at Ur and Kish; and the newly-found and most unexpected Indus civilization at Harappa and Mohenjo–Daro; ending with an exordium on the relation of Europe with the East.

I do not intend to recapitulate Prof. Childe's views here: they must be ascertained by reading his book. I find little to quarrel with except on minor issues. We must wait for more light. Naturally there is no finality about any of these things yet. He is very definite on Susa I and II; but I prefer to suspend judgment still on the very interesting question whether it is Susa I or II that is contemporary with al-'Ubaid, and what the relation of the pottery of Jemdet Nasr is to that of al-'Ubaid* or that of Nihavand to Susa. On p. 166 Prof. Childe seems to bring Semites as ' a new ethnic element ' into Elam with the second Susian style. Early (pre-Sumerian) Semites in Sumer are not impossible, though I do not think them very probable; but of Semites in Elam we know nothing. They left no linguistic traces of their presence there, at any rate, and Semites without a Semitic language are no longer Semites: properly speaking the word ' Semitic ' denotes a language, not a race. We cannot speak of Semitic skulls.

The author is interested in the disputed matter of the early relationships of Sumer with Egypt and the primacy of the one culture over the other. He seems generally to agree with my own contention that Sumer is probably the older: that at any rate of the nearly contemporary cultures of early Ur and First Dynasty Egypt the former is the more highly developed and seems to have communicated certain ideas to Egypt, which, as I

* The latest results from Ur should decide this point.—ED.

believe I was the first to point out, appear exotic in Egypt and eventually died out there. One does not thereby ignore the predynastic culture-history of Egypt, as one is accused of doing. I entirely agree with Prof. Elliot Smith that the predynastic Egyptian culture is one with that of dynastic Egypt ; the dynastic culture grew up as definitely from the predynastic as the Minoan culture grew out of that of neolithic Crete : there is no break. But I do think, and Prof. Childe is inclined to agree with me, apparently, that early Egypt borrowed some elements of civilization from the culture of Sumer, which *c.* 3000 B.C. was more highly developed than that of Egypt, and so was probably the older of the two. That is all and this conclusion is reinforced by the fact that one can see nothing in Sumer that was borrowed from Egypt : that is to say the Sumerian culture was probably more fixed and characterized than that of Egypt *c.* 3000 B.C., and so again was probably the older. Time will show if this is right or not.

Prof. Childe well brings out the vital difference between the geographical situation of the Egyptian and the Mesopotamian river-valleys (p. 125), which had a great influence on their respective histories. Perhaps he over-emphasizes the dissimilarity of Egyptian and Mesopotamian religion (p. 194) : there are many real resemblances such as that which existed in some respects, and not only philologically, between Osiris and Ashur (Marduk), and still more such as that between the goddesses Hathor and Ishtar, notably in the latter's Sumerian form Ninkhursag, 'the Mistress of the Mountain'. The likeness of Ninkhursag to Hathor in all respects—as cow-goddess, as mistress of the desert, as protectress of the necropoles, is almost uncanny. I may refer to Mr C. J. Gadd's remarkable chapter on al-'Ubaid on the subject. Shall we look to Syria for a common origin for both?

The most interesting question of all is now for the first time presented to the public in an accessible form : the question of the early Indus civilization. It presents many difficulties. When, in 1913, I first suggested that the Sumerians bore, to judge from their sculpture, a physical resemblance to Indian types, chiefly of the Dekkan, and so probably Dravidian, I never expected that an early civilization would be unearthed on the banks of the Indus which shows unequivocal evidence of connexion with Sumer. Yet a trade-connexion is conclusively proved by the discovery at Ur and elsewhere in Babylonia of signets of the same type as those found in India and with the same foreign writing as those from India : nay more, by the find at Ur of an Indian seal, with an Indian bull-figure on it, but with a cuneiform inscription instead of an Indian one ! And there are resemblances in other things : the figure of a bearded man from Mohenjo Daro is very Sumerian in character, and the trefoil marks on his garment are precisely the same as the markings of ox-hides in Egyptian and Cretan representations. But the pottery seems different ; and is claimed to be wheel-made at an epoch older than that of Sumer. The chariot too is claimed to be older in India than in Sumer. Will some Indian archaeologist claim that all culture came, not as Prof. Elliot Smith thinks, from Egypt to Sumer and India, but from India to Sumer and eventually Egypt ? *Bande Mataram !* Prof. Smith must indeed look to Egypt's laurels, if this goes on !

In fig. 8 Prof. Childe presents a very interesting early Indian wall-painting in which a rhinoceros-hunter is shown using triple-barbed spears of a type that is known from India in copper. There is an incomplete example in the British Museum prehistoric collections, and a fine one is published in *Proc. Soc. Ant. Scot.*, VIII, 293, X. 691, p. 72. Oddly enough, it was found on the banks of the Tweed near Norham, where it must have been thrown away by some returned traveller from the East or some person to whom he had given it. It is referred to the beginnings of the age of metal in India. But the

finest example known is that in the possession of Captain E. G. Spencer Churchill at Northwick Park: this has, too, a magnificent patina.

Prof. Childe has given us the pith of the new finds in his chapter on the Indus Civilization, which all may read and perpend. And we may go on perpending for some years yet. The time for the death of theories and the birth of facts is not yet in this matter. But it is all extraordinarily interesting. One thing to be said (with bated breath, perhaps) is that we hope the excavators will soon give us a temporary rest from these amazing discoveries. We want a sabbatical year or two in which to digest them. As it is we are too busy keeping up to date to be able to sit down and consider them at leisure.

As to details, I do not see that lapis-lazuli is 'far-away' in Seistan (p. 147): it is found I believe almost exclusively near there, in Badakshan. Ape-statuettes (*ibid.*) in Babylonia may mean an Indian as much as an Egyptian connexion. Were lip and nose-plugs found at al-'Ubaid (p. 139)? I know what Prof. Childe is referring to: but may some not equally well be ear-plugs, and others (the pegs) be merely just pegs, and not intended for human decoration at all? I continue to question (p. 191) the deliberate partial cremation of Sumerian bodies, though I am aware that Woolley considers he has some further reason for supporting it. In house burials there is always the possibility of burials being burnt in a general conflagration of such a city as Ur, and in deep graves there is the curious phenomenon of slow chemical carbonization to be taken into consideration, which is illustrated often in Egyptian mummies, most notably in that of Tutankhamen. There is no trace of even partial cremation in cuneiform literature. But, neither, on the other hand, is there any trace in it of the undoubted massacres of slaves and soldiers that took place at Ur.

There are several misprints: *e.g.*, 'brousing' (p. 24), 'aberrent' (p. 28), 'apoge' (p. 123), 'Erivain' (p. 136), 'theriomorphis' (p. 153), 'heirarchy' (p. 170), 'Shub-ad' for either Sub-ad or Shub-ad (p. 193). And why use the German form 'Jenisei' for the Yenisei (p. 40)?

H. R. Hall.

REPORTS OF THE RESEARCH COMMITTEE OF THE SOCIETY OF ANTIQUARIES OF LONDON. No. VII. Second Report on the Excavation of the Roman Fort at Richborough, Kent. *By* J. P. Bushe-Fox, F.S.A. *Oxford University Press.* 1928. *pp.* 231 *with* 47 *plates.* 7*s* 6*d.*

THE ROMAN FORT AT OLD KILPATRICK. *By* S. N. Miller, M.A. Jackson Wylie & Co. 1928. *pp.* XVII, 63; *with* 27 *plates and* 3 *figs.* 12*s* 6*d.*

Excavations undertaken by the Research Committee of the Society of Antiquaries have a way of producing masses of invaluable material for students of Romano-British archaeology, and the present report (which carries the record of the excavations at Richborough up to the end of 1925) is perhaps even more noteworthy in that respect than its predecessors. Much has happened at Richborough, so that it is not necessary to summarize here the results included in this report, but particular attention may be directed to the coin-list (which records as many as 17,000 coins), and to the sections on small objects and on pottery, and to the plans and sections, which are the best we have seen in any excavation report. How the report can be published at the price we do not know; no student of archaeology can afford to be without it, and it is within the reach of everyone. No country has yet produced a report that can compare for accuracy, clearness, and *format*, with this and its predecessors, and British archaeology owes a great debt to Mr Bushe-Fox and his colleagues for their work.

Mr Miller, at Old Kilpatrick, was faced with a far more troublesome task than the excavators of Richborough ; there, excavation proceeds steadily and surely, supervised by a large staff of specialists. Mr Miller was compelled, in two short seasons, to rescue what he could of the Roman fort, before the builders destroyed it ; and the work of supervision and reporting on the finds (lamentably few as they were) was left almost entirely to him. In spite of the scarcity of structural remains, and of pottery and coins, he has succeeded in securing a very complete picture. First, an Agricolan *praesidium* (though perhaps the pottery evidence that he cites is hardly sufficient to warrant the supposition that that fort was occupied for more than a season) ; and then the Antonine fort, founded before the new Wall was begun, and associated with a port through which the materials needed for that work passed ; together with evidence for the same three periods in the life of the fort as had previously been noticed at other sites in Scotland. As yet, no attempt seems to have been made to find an explanation of the second reconstruction, except as ' an incident in the final abandonment '. It may be worth suggesting, in view of the latest results at Birdoswald, that it was the Scottish Wall that was restored by Ulpius Marcellus, and that the complete abandonment did not occur before Clodius Albinus crossed to Gaul for the final clash with Severus.

The report is well printed, but the line drawings are not as good as they might be.

These two reports are excellent examples of two of the classes of excavation that are most needed now—the thorough excavation of a site important for the whole history of Roman Britain, and the investigation of a site threatened by the builder. Each might be taken as a model of its kind by excavators faced with similar work, and each is an indispensable work of reference for students of Romano-British archaeology.

E. B. Birley.

NORTHERN NEǦD : THE MANNERS AND CUSTOMS OF THE RWALA BEDOUINS. *By* Alǒis Musil. *American Geographical Society, Oriental Explorations and Studies, nos. 5 and 6. New York.* 1928. *pp.* 368, *with* 67 *illustrations and map.* 7 *dollars.*

These further instalments of the monumental work of Musil, for the publication of which we have to thank the munificence of Mr Charles R. Crane, deal more exclusively than did their predecessors with the modern aspects of the Arabian desert, and appeal primarily to the geographer and the ethnologist. Very few ancient sites were encountered in the course of the journey which Musil describes with his usual minuteness of detail in the first part of the volume on Northern Neǧd ; to the archaeologist the appendices will prove more interesting than the text proper. The history of the houses of Eben Rašîd and Eben Sa'ûd are recorded at length, and the author has shown an industry in amassing information which is only equalled by the clearness with which he sets it in order. Recent as most of the events are, they illustrate desert conditions which are of all time, and if in 1824 an Eben Sa'ûd was at war with a Fejsal eben Dawiš, sheikh of the Mtejr, and today Eben Sa'ûd is in the field against the same tribe under another Fejsal eben Dawiš, we can be sure that the literal repetition of events is no unique thing, but that the modern history of Arabia may well be called in to fill up the gaps in the scanty records of its past. Particularly interesting are Musil's views on the alleged desiccation of Arabia in historical times as being the reason for the migration or expansion of warlike tribes. To this theory Musil is strongly opposed, holding that the changes in the fertility of the country have been always due to human destruction or neglect and to the use or abandonment of trade routes. At the present time Arabia is as well watered and

as capable of maintaining a large population as it has been at any period in history. His opinion is certainly supported by facts in Palestine and in the Sinaitic desert south of Palestine. In the latter area town ruins speak of flourishing conditions in the fifth century A.D., whereas today only a few impoverished Bedouin wander over a barren waste ; but everything in the ruins, the use of stone rafters instead of timber, the existence under every house of a stone cistern which took all the drainage of roof and court, the elaborate system of dams and catchments and rock-cut cisterns all over the country, prove that the rainfall was no greater then than now but was scientifically utilized. The Moslem conquest destroyed the towns, the stoppage of the silk trade along the Akaba route took away their *raison d'être*, and Sinai reverted to desert. That the same thing on a larger scale was true of Arabia is most probable. Musil cannot accept, for geographical reasons, the identification of the Tejma oasis with the Tema where Nabonidus established the seat of his Babylonian empire, and here too the opinion of a first-hand observer must carry weight. On p. 226 al-Mkajjer is wrongly given instead of Warka as the modern name of Erech ; the correct identification with Ur occurs on p. 307.

The second volume deals with all aspects of the life of the Bedouin tribesman, the descriptions being throughout illustrated by poems which Musil took down in writing from the singers and translates and annotates most fully. Nothing so detailed and systematic has been attempted by any other writer on this country, and though the book is not easy reading it is a veritable storehouse of information, nor is it likely ever to be supplanted.

C. L. WOOLLEY.

ANNUAL REPORT OF THE ARCHAEOLOGICAL SURVEY OF INDIA, 1925–6. *Edited by* J. F. BLAKISTON. *Calcutta : Government of India Central Publication Branch.* 1928. *pp.* XV, 306, *with* 79 *plates.* 53*s.*

The pleasing theory that all things bright and beautiful, especially ourselves, are of ' Aryan origin ' has been rudely shaken since Schliemann began to clear the rubbish, material and academic, that had accumulated over the site of Troy. Sir Arthur Evans' discoveries in Crete knocked the bottom out of the old controversy whether Celt or Teuton is true heir to the ' Aryan ' halo, and now Sir John Marshall and his officers bid fair to prove that Indian culture is, after all, of Indian origin. Interest in ancient India accordingly centres at present in India's challenge to nordic theories, the so-called ' Indo-Sumerian ' culture (pp. 72–98), associated as it is with silver, tigers and other things, unknown apparently to Rigvedic Aryans.

In the season under report Sir John Marshall concentrated his efforts on Mohenjo-Daro ; excavation proceeded in five different areas, while Mr Hargreaves was deputed to dig the Sohr mound, near Nal in Baluchistan, in the hope of discovering early culture contacts between Sumer and the Indus valley. It so happens, however, that the results of these—and later—excavations have already been summarized by Sir John in *The Times* and *Illustrated London News* in 1926 and 1928, and this Report does not add much to the evidence and inferences so published. (See ANTIQUITY, 1928, II, 83, 84).

In his Report for 1923–4 Sir John deprecated the ' wild writing ' that greeted his first announcement of these discoveries in the London press, and declined to be jostled into premature publication. His caution was justified, for the evidence is not so easily unearthed and appraised as some sanguine spirits anticipated. Already the term ' Indo-Sumerian ' has to be dropped ; it presumed too much, and ' Indus ' culture is safer ; and the Nal excavations,[1] which suggest affinities with Susa and Musyan, reveal no certain

[1] Since published in Memoir no. 35 of the Archaeological Survey, 1929.

connexion with the Indus sites. Though 'five distinct strata' have been exposed at Mohenjo-Daro, they cannot, so far as this Report goes, be culturally differentiated. In fact, the canons by which archaeologists in other fields build up their chronology are in India so far lacking. The form, fabric, decoration and technique of Indian pottery and beads has not yet been exhaustively examined and classified. The significance of associated fauna and flora, of breeds of domesticated animals and plants particularly, deserves more attention than it receives. A few analyses of minerals and metal artifacts are recorded, but their provenance is not established; while copper celts, which are said to link the Indus culture with the famous finds at Gungeria in Central India, are not recognizably figured (pl. XXXVIII, f.). Even the technique of building construction, which yielded such useful results in Taxila, seems to fail in Mohenjo-Daro. No doubt these matters will be clarified in due course when the promised Memoir is issued; the volume under review is, after all, an official report and not a monograph.

Two other sites of interest were dug during the year, viz. (1) Paharpur in Bengal, which yielded remarkable evidence of the eastward extension of Gupta art, and (2) the famous Buddhist monastery of Nalanda in Bihar. An account of these also has appeared in the *Illustrated London News* (1928). The only other points of note in the Report are a brief summary by Sir Aurel Stein of his successful venture on the track of Alexander, a few useful notes on iconography and a cursory account of some very interesting inscriptions.

India is well ahead of the rest of the Empire in recognizing the national importance of national monuments. The Government have been generous, the staff able. These annual reports are superbly printed; the illustrations are beyond all praise. To criticize such munificence seems ungracious, but it fails to satisfy because it attempts too much.

Of the 306 pages of the Report roughly one-third is devoted to archaeology, two-thirds to departmental administration. There is no index, no map. The archaeological sections contain many appetizing morsels which require to be served separately as memoirs to be properly digested. From the administrative sections also some facts may be gleaned the scientific value of which would be clearer if they were scientifically arranged. But the embodiment of 93 pages of accounts in a volume *de luxe* is an outrage. Presumably the Editor is the victim of conflicting forces, the demands of science and the prescriptions of a secretariat. None will deny the importance of annual reports, careful finance, and, above all, conservation. But the present system is wasteful; two-thirds of the letter press are of no permanent value whatsoever; of the rest much will be reprinted in the memoirs; and meanwhile the Report itself is delayed by its own complexities and the publication of material of urgent scientific importance is held up. Till 1915–16 the administrative and scientific aspects of the Survey were dealt with in separate publications. A reversion to that system should free the funds and brains of the Department for the work that really counts.

Meantime the educative value of the Survey suffers. Archaeology needs the support of public opinion, and the public, both in India and in England, is receptive. Publicity is not to be despised; small hand-books, such as Sir John Marshall's guides to Taxila and Sanchi (1918) or Mr Longhurst's *Hampi Ruins* (1917)[2] serve a very useful purpose, and Sir John's timely communications to the London press are by no means the smallest of the many services he has rendered to the cause of Indian Archaeology. F. J. RICHARDS.

[2] The *Guide to Sanchi*, the Report says, has been sold out, and only Urdu copies are available. Of *Hampi Ruins* a second edition is announced.

MACEDONIAN IMPERIALISM AND THE HELLENIZATION OF THE EAST.

By Pierre Jouguet. *Kegan Paul, Ltd.* 1928. *pp.* 440, 7 *plates*, 4 *maps and plan.* 21*s.*

In this valuable addition to the 'History of Civilization' series, M. Jouguet deals with the complex world that arose as the result of Alexander's conquests. 'Hellenism conquered the East by means of the armies of Macedonia and its own institutions. It is the history of that twofold conquest that this volume has attempted to trace'. Thus in his conclusion the author summarizes his aims, and adds, with a consciousness of the poverty of his sources and the difficulty of the task, that 'it was hardly possible to succeed'. Yet if complete success in such an enterprise is not to be looked for in the present state of knowledge, the book gives an admirable survey of what is known, and of many problems that await further evidence for their solution, such as the exact part played by the 'politeumata' in Egypt.

The book is divided into four parts. The first deals with Alexander's conquests and the organization of his empire as far as it can be determined in the incomplete state in which it was left at his death. In part II the dismemberment of the empire is described and the succeeding conquests, until in 281 B.C. the assassination of Seleucos removes the last of the generals of Alexander, and the Eastern Mediterranean settles down under the three powers which are to dominate it until Rome becomes supreme. The third part is concerned with the rivalry of these powers and traces the fortunes of the Lagid and Seleucid empires down to the beginning of the 2nd century B.C., when the second Macedonian war and the fall of the Seleucid power mark the decisive intervention of Rome in the East and the beginning of a new epoch. M. Jouguet has some penetrating remarks on the causes of the rivalry between the Hellenistic monarchies and discusses their need for Greek settlers in their policy of Hellenization, the advantages of the control of the coasts and islands of the Aegean, and their general economic situation. 'The Hellenic Mediterranean and the Eastern world, which had never been separate, now formed a more complete unity, since the same civilization covered the whole, if unequally. This intellectual and moral unity was reinforced by economic ties. Between Asia and Eastern Europe trade had always been considerable, and this was what had, for example, made the prosperity of the old Greek cities of Asia Minor. It now enjoyed greater facilities than ever. Warlike expeditions and geographical exploration had brought a better knowledge of the great trade-routes which crossed the heart of the Asiatic world to the Far East, they had opened new routes, and, above all, they had revived traffic on roads which were forgotten or partially abandoned . . . But the Empire was divided up, and the rival kingdoms, quarrelling for the leadership, were naturally inclined to quarrel for the control of the trade routes, and especially for the ports at which they ended on Hellenic waters, for these were a great source of wealth, and wealth was necessary for the conquest of power'.

The fourth and concluding section of the book deals more specifically with the theme of the Hellenization of the East and the organization of Hellenism in the Graeco-Oriental kingdoms. In this, perhaps the most interesting and important part of the book, M. Jouguet devotes the greater part of his space to Egypt 'because', as he remarks, 'the historian is dependent on his sources and Egypt is the country which has preserved the most evidence, and the most precise, about its past'. The papyrus finds have given to the historian a mass of varied documents which afford glimpses of the inside life of this country, and throw light on matters which can only be conjectured in the case of the other Hellenistic monarchies.

In the light of these documents M. Jouguet considers the organization of power both central and local, the position of the god-king, the administration of justice and the means by which the Ptolemies sought to spread Hellenism in a country whose manners and institutions were so alien to the Greek way of life. By the spread of Greek education in palaestrae and gymnasiums Greek traditions were fostered, and ' the idea of Isocrates was applied that it is not blood but education that makes the Hellene '. Similar aims probably influenced the treatment of agriculture and the allotment of land. ' The policy of the Lagids ', says M. Jouguet, ' aimed at creating between the fellah in the country and the aristocracy of the cities and court, a mixed population, which might be penetrated with oriental ideas, but, in the higher classes, was dominated by Hellenic culture '. M. Jouguet's book opens with the spectacular achievements of Alexander and closes as the power of Rome becomes established in the East. To the student of the Roman Empire and of Greek history alike this period is of great interest and importance. Rome succeeds to many of the problems which faced the Hellenistic monarchs, and is in turn influenced not only in all probability by their methods of organization but by the welter of ideals and ideas, Hellenic and Oriental, with which the Eastern world was already saturated. M. Jouguet is to be congratulated upon a volume which contains not only a lucid narrative of events but much that is stimulating and suggestive. Following the usual excellent practice of this series the book contains a bibliography of some 240 volumes to which reference is made throughout the text. There is also an index. G. F. Forsey.

SIX CAUSERIES FAITES À RADIO-BELGIQUE. *Par* Jean Capart, *Conservateur en Chef des Musées royaux du Cinquantenaire.* 1926.

BELGIQUE ANCIENNE : guide du visiteur. *Par* le Baron de Loë, *Conservateur.*

GUIDE SOMMAIRE DES VISITEURS DES MUSÉES ROYAUX DU CINQUANTENAIRE. [*Par* Mlle. Yvonne Dupont]. 1928.

GUIDE DU VISITEUR : Egypte. *Edition de la Fondation Reine Elisabeth.* 1928.

These four little booklets arrived for review at an opportune moment when the reviewer was about to visit Brussels for the express purpose of seeing the Cinquantenaire Museums, where the national archaeological collections are housed. Both the guides and the visit revealed a spirit of enthusiasm which is not always associated with state enterprises. Criticism is disarmed by this fact, for such shortcomings as are to be observed are mainly due to factors beyond the control of the conservators. That so much has been achieved during the past quarter of a century is evidence of the triumph of mind over immense material difficulties.

The collections consist of prehistoric Belgian antiquities of Belgo-Roman remains (the latter surprisingly rich), and of classical and ancient Egyptian relics. The excavations of Professor Sir Flinders Petrie are well represented. The arrangement of the Belgian section is excellent ; one begins with the palaeolithic period and proceeds, through a rich Omalian neolithic, to the bronze, iron and later periods. There are models of Roman villas, and some wonderful objects from the Belgo-Roman burial-mounds, many of which are found (as in England) beside Roman roads. A composite photograph of an old map of 1693 (published in the *Histoire militaire des Flandres*, by the Chevalier de Baurain) is

exhibited, showing a whole chain of them along the Chaussée Brunéhaut from Bavay to Tongres. On the wall is a series of large maps showing the distribution of remains of the palaeolithic, neolithic, bronze, iron, Belgo-Roman and Frankish periods. The choice of symbols is not ideal, but the chief habitation-areas in each period are well brought out, and that is after all the main thing.

The principal neolithic sites are of course the flint-mines of Spiennes ; there is also a collection of objects from the Omalian village of Vaux-et-Borset near Liége. The region of Hesbaye was the most northwesterly point attained by the Danubian neolithic ' bandkeramik ' culture ; it is best studied however in the Liége Museum (closed on Saturdays), which houses the very rich finds from the excavations of MM. de Puydt, Hamal Nandrin and Jean Servaix. One does not usually think of Belgium as a dolmen-country, but there are several in the hill-country of the southeast ; one (of which a model is exhibited) at Weris has a well cut door-like entrance in a huge slab at the end.

The late Bronze Age cemetery of La Quenique contains a large number of globular cinerary urns with Lausitz connexions eastwards and perhaps British (Devrim) connexions in the west. The bronze objects associated consist of bronze razors, pins and swords.

A Hallstatt burial at Eygenbilsen (Limbourg) contained a bronze ewer of Italian origin (see ANTIQUITY, II, 428 and pp. 130–2 of the present volume), a cordoned bronze situla and a fine golden diadem.

There is a model of the hill-fort of Hastedon at Saint Servais (province of Namur). There are several other hill-forts in Belgium, some of which are said to be vitrified (Baron de Loë, p. 32). We tried to locate the remains found in them but were unable to do so. It is unfortunate that the arrangement (and apparently to some extent also the numbering) of the cases does not correspond with that of Baron de Loë's guide, of which a new edition is required.

The Iron Age potteries recently discovered at La Panne fill a fine case. Many of the pots are perfect and the whole is a most valuable collection. Certain objects (like our ' hand-bricks ') are explained as supports for use during firing. An Iron Age lake-village at Neckerspoel (the name is suggestive) near Malines yielded a few kinds—enough to show that it was inhabited during the La Tène period.

M. Capart's radio-talks are an admirable exposition of the functions of a national museum, written in popular language and eminently readable. In his first talk he asked listeners to send him a postcard saying why they had never been to see the museum, their national heritage. The replies are instructive ; twenty-three only were received. (Four who lived at Brussels had never heard of the museum !) A casual encounter in a tram was responsible for the surprising opinion that museums were yet another device for finding well-paid jobs for *fainéants* ! —which shows how little one-half of the world, or more, knows how the other half lives.

Full information about the times of opening, etc., is given on p. 8 of the Summary Guide. The Museum is closed on Fridays. In passing we may ask why an international agreement cannot be made with regard to the closing-days of museums. Here is a small but important task for some international body—to attempt to secure uniformity of opening and closing times ; or at any rate to compile and publish a handbook of information. Why, for example, does not the Intellectual Cooperation Committee of the League of Nations take cognizance of archaeology, one of the most lively and most international branches of science ? Why are all international rapprochements left to overworked individuals ?

MEMOIRS OF THE ARCHAEOLOGICAL SURVEY OF INDIA. No. 41. Survival of the Prehistoric Civilisation of the Indus Valley. *By* RAMPRASAD CHANDA. *Government of India*, 1929. *pp.* 40, *and* 2 *plates, index. Rupees* 1-2 *or* 2*s.*

It is difficult to understand why this volume was published by the Archaeological Survey of India, for it consists almost entirely of extracts from Sanskrit literary sources which have no bearing whatsoever on archaeology. Of course Mohenjo-Daro and Harappa provide the excuse ; they are in the Indus Valley and so, at one time, were the peoples of the Vedas. Unfortunately, it is all a little vague. Until the long expected Indus Valley excavations report is on the table, it must be confessed that we know extremely little about Mohenjo-Daro and Harappa. It requires a very superficial acquaintance with Sanskrit literary history to realize that the earlier and later Vedic ' periods ', upon which Vedic chronology hangs, are comparative terms, and little else. Dates, of course, are frequently given—in good round numbers. Since, however, there are no dated sources at all, the chronology of Vedic literature can only be compared to a very indigestible sandwich—a great deal of opinion crammed between a lower limit provided by the so-called Indo-Iranian schism, and a higher limit provided by the rise of the Buddhist and Jain traditions. It is evident that the mass of opinion here set out is based upon some sort of estimation or guess-work as to the probable rate of development of the language. How it is done is a mystery, for it is an acknowledged fact that languages have a way of lying dormant for long periods and suddenly bursting into a new phase ; modern English and modern Persian are excellent examples. Such galvanic activity must have causes—and external, concrete causes at that,—causes which are truly cultural and which, unless represented culturally, must remain unknown. It is evident in such cases that the literary student can only state his opinions and wait for the archaeologist to arrive at the facts. In this volume the process is reversed. Text after text is thrown up on a number of subjects, such as human sacrifice and *Sati*, and, at the end of it all, we are told that the well-known Mohenjo-Daro stone heads prove the practice of *Yoga* in the Indian chalcolithic period. The evidence is the squint in the eye.

The Archaeological Survey is a Government Department and doubtless has its own troubles. But why should either the Survey or the Government of India insist on publishing this kind of pointless work when there is so much unpublished that scholars would universally welcome ? The Survey is a national institution, but it is also a scientific institution. Strong co-operation is waiting for it, whatever its difficulties are—if only it will co-operate. K. DE B. CODRINGTON.

PAPERS ON THE ETHNOLOGY AND ARCHAEOLOGY OF THE MALAY PENINSULA. *By* IVOR H. N. EVANS, M.A. *Cambridge University Press.* 1927. *pp.* VIII, 164 *with* 43 *plates.* 15*s.*

This is a collection of papers previously published in local periodicals of Malaysia. They do not belong to a definite plan and are therefore rather scrappy. This applies particularly to the articles on the Pagan races. They appear quite untouched by the example of Rivers, whose great merit it is to have always insisted on the ' concrete method '—that is the exhaustive study of single communities as the preliminary to a general survey of allied ones. We should have preferred such an intensive study of one pigmy community ; but we are given instead mere fragments hastily collected through an interpreter. Such scraps were useful in earlier days as a starting point,

but they have been rendered very inadequate since intensive study has become more general. It is impossible to give a useful account of ' the religion and customs ' of the Negritos of peninsular Siam in two pages (12-14) as the author attempts. The author's bent is not really in that direction, but in that of technology. The series on Malay and other technology is made up of detailed and careful descriptions of objects and technique. It is interesting to note that the Malays, like the Sinhalese, first shape their pottery on the wheel, then beat it out with a mallet. It is with a grooved mallet that the patterns described on p. 123 and illustrated on plate XXIX are made, ' not by the application of fairly fine cord '. This type is common in Ceylon about the 10th to 12th century, and probably long before that. I fell into the same error as the author until I saw the method actually in use and had larger specimens to examine.

It is evident that Malay archaeology suffers, like that of India and Indo-China generally, from the neglect of pottery. Hence the fact frequently recorded that the miscalled 'corded pottery' occurs with stone implements (pp. 141, 144, 151) does not help us at all. Yet it is crucial. The Sarasins also found pottery with quartz implements in Ceylon, but were so persuaded that they did not belong together, that they did not, so far as I am aware, trouble to describe the pottery. The author's finds in Malaysia leave no doubt that they do belong together, and in Malaysia it is a type of pottery that was common at a fairly late period in Ceylon.

The author is handicapped in his archaeology by his lack of familiarity with Buddhist and Hindu archaeology. The fact is that he has far too big a task set him. It is impossible for one man to do both the anthropology of Malaysia and at the same time run a museum ; and all this without, apparently, an adequate staff of draughtsmen—for to such a lack we must attribute the insufficiency of maps and drawings. For instance a verbal description of pots on p. 71 is a very inadequate substitute for drawings. It is to be hoped that the Government of Malaysia will soon come to realize that if the work is to be done at all it must be done properly, and that can only be done by portioning it out among an adequate number of workers with their special qualifications.

A. M. HOCART.

LES ANTIQUITÉS BOUDDHIQUES DE BĀMIYĀN. *Par* A. GODARD, Y. GODARD *et* J. HACKIN. Tome II. *Van Oest.* 1928. *Price not stated.*

This second volume has preceded the first, which will deal with the origins and itinerary of the expedition. Bāmiyān is a town in the Hindu-Kush, north of the main range, about 90 miles from Kabul, the capital of Afghanistan. It lay on one of the most important roads that in antiquity connected the Oxus watershed with the Indus, and was, from the early centuries of the Christian era until the Muslim conquest in the 8th century, an important centre of Buddhism. A Buddhist temple with its idols intact apparently stood here as late as 871, when these idols were brought to Baghdad. The existence of two colossal images, cut out of the face of the cliff, had been known to European writers since the 17th century. But it was not till the visit to Bāmiyān of M. Alfred Foucher in 1922 that they were studied by a specialist in Indian archaeology. The preliminary observations of M. Foucher were completed by M. Hackin and his colleagues in November 1924. The older of the two giant Buddha-images (known as the 35-metre image) seems to date from about the 2nd century A.D. In connexion with these figures are a number of cave-shrines, some of which contain remains of paintings. The caves thus decorated date from the third to the beginning of the fifth centuries,

and are thus the most ancient Buddhist wall-paintings, apart from caves 9 and 10 at Ajanta and the paintings from Miran in Central Asia. The costumes of the donors who appear in these paintings (particularly a coat with broad lapel on right side only) and many other details connect them with the Iranian rather than the Indian world. The actual cult figures are, however, prevailingly Indian. What M. Foucher calls the Chinese element may, in fact, represent the Indo-Scythain (Kushān) contribution to the style. For the ' ancient king ' who built (according to the Chinese 7th century traveller Hsüantsang) the first monastery at Bāmiyān was probably Kanishka, the great Kushān monarch ; and it was from the same Indo-Scythians (called by them ' yüeh-chih ') that the Chinese got their first lessons in Buddhism.

The latter part of the book deals with the remains of a vast composition painted on a wall of rock at Dokhtar-i-Noshirwan, near Rui, thought to have been executed for one of the Sassanian governors of Bactria about the 6th century. Only fragments are still decipherable ; but it is plain that here the painters, who may themselves have been Buddhists, carried out a scheme of decoration largely consisting of traditional Iranian motifs, though with some use of their own repertory.

There is a valuable appendix on ' Bāmiyān dans les textes chinois ', by M. Paul Pelliot.

ARTHUR WALEY.

SEGOBRIGA. *Von* Prof. Dr ADOLF SCHULTEN. *Sonderabdruck aus d. Deutsche Zeitung für Spanien*, 25 March and 10 April, 1929. *Barcelona. pp.* 20.

The purpose of this article is less to praise the very excellent article under review, than to draw attention to the Corpus of such topographical studies, all from Dr Schulten's pen, which has grown up since 1919. During the last decade, the *Deutsche Zeitung für Spanien* has published year by year encyclopaedic studies of ancient Roman, Greek and Iberian sites in Spain ; all are worth reading, and rank in value with Dr Ashby's similar studies in Italy, as first-class accounts of South European sites that lie off the beaten track, and are little known: accurate, concise, summaries of the archaeological material. Here is a list, compiled with the aid of Dr G. F. Hill.

1919-20 : Ibiza, 10 and 25 Aug., 1920 ; Tarragona, 10, 25 Jul., 10 Aug. 1922 ; Meca, Merida, 1922 ; Die älteste Beschreibung der Pyrenaeen-Halbinsel, 25 April, 10 May, 1922 ; Tartessos, 25 Dec., 10, 25 Jan. 1922-3 ; Coto de Doñana, 25 May, 10 June, 1923 ; Gades, 25 Jul. 10, 25 Aug. 1923 ; Die Schlacht bei Munda, 25 Oct., 10 Nov. 1923 ; Avila, 10, 25 Feb. 1924 ; Mainake, 10 Jul. 1924 ; Cariño, 25 May,1925 ; Cauca, 10 Apr. 1927 ; Cáceres 10 Apr. 1928 ; Segobriga, see above.

I. A. RICHMOND.

JAHRBUCH DES PROVINZIAL MUSEUMS ZU HANNOVER. *Neue Folge, band* 3. *Hannover*, 1928. 172 *pages*, 36 *plates*.

This well got-up publication—it is printed in Latin characters—reports fully on the activities of the year 1927, the 75th of the museum. It begins with an obituary notice of Dr Adolf Fritze, a well known German zoologist and former director of the Hanover Zoological Gardens, and an active member of the museum staff. The Director of the Museum in general, and of the prehistoric and ethnographic section in particular, Dr K. H. Jacob Friesen, is probably the best known to the world outside Germany.

The *Jahrbuch* gives a short account of the earlier days of the museum, of the dispersal of private collections in the 18th century, and the adventures that happened to some

of the present exhibits. There were for instance some Roman busts which George I of England acquired on the death of Louis XIV, through his ambassador in Paris. The French retrieved these busts in course of the Napoleonic wars, but in 1813 fourteen of the 25 were brought back to Hanover. The authenticity of these busts as real antiques seems somewhat doubtful, but the tale of their adventures is worth telling.

The *Jahrbuch* describes the present arrangement of the Provinzial Museum, of which the prehistoric section should interest us as containing much that concerns the ancestors we have in common with the folk of Hanover and the country between that city and the sea. The compilers of the *Jahrbuch* have devoted two-thirds of the space at their disposal to a reprint of Mushard's *Palaeogentilismus Bremensis*, a work which shows the methods of prehistorians of nearly 200 years ago.

Martin Mushard was born at Bremen in 1699 and spent his long life as pastor in the lands between the mouths of the Weser and Elbe. Unhappily nothing is known of the many finds he describes; they seem to have got into private hands. But his work, republished in the *Jahrbuch*, stands as a monument to one who was in his day the most prominent of North German prehistorians. Indeed the loss of Mushard's many finds is qualified by the excellent drawings with which he illustrated his catalogue, and the *Jahrbuch* reproduces these with great care and clearness. The text of Mushard's work is given almost literally in archaic German with a northern idiom, and this should prove of interest to philologists. For medical men a Doctor Major Rhodius, who collaborated with Mushard, prescribes the ' green rust ' scraped from metal finds, as ' Präsentissimum remedium ' for intermittent fever. The reading of Mushard's work thus reproduced in the *Jahrbuch*, arouses a desire to revisit that land of shifting sands, tidal rivers and wide heaths which some of his remote ancestors exchanged for much the same surroundings in our country. B. GRANVILLE BAKER.

GUIDE ARCHÉOLOGIQUE AUX TEMPLES D'ANGKOR. *Par* HENRI MARCHAL. *Paris and Brussels : Van Oest*, 1928. *pp.* VII, 217, *with* 16 *plates and map. Price not indicated.*

It is unfortunate that this excellent handbook was written just at the moment when M. Philippe Stern was putting forward a theory which entirely upsets the hitherto accepted chronology of Khmer art. The old chronology has now been abandoned even by M. Parmentier, who was to a large extent its author. A summary of the new views will be found in Grousset's *Histoire de l'Asie*.

The speculative mind will certainly be stimulated by this volume to examine once more the problem raised by the extraordinary resemblance between the Angkor architecture and that of certain Central American ruins. Imbelloni, in his *La Esfinge Indiana** (1926), plate IV, has already correlated the Phimeanakas (9th century, the central shrine of the original Angkor Thom group) with the stepped pyramid at Papantla, in Mexico. The latter is supposed to date from the 11th century. It is hardly credible that such a resemblance is fortuitous ; but so long as chronology, on both sides of the Pacific, remains in its present state of fluctuation, explanatory theories are hardly worth forming.

Angkor Vat, the later temple, just outside the precincts of the royal town, is now placed *c.* 1112–1152. Comparatively recent research has shown that Khmer art did, as was formerly supposed, suffer a sudden extinction after the 12th century, for the temple of Banteai Srei, 14 miles from Angkor, dates from the 14th. A. WALEY.

* Reviewed in ANTIQUITY, I, 241–3.

THE HISTORY OF THE DEVIL: the Horned God of the West. *By* R. Lowe Thompson. *Kegan Paul.* 1929. *pp.* 168, *illustrated.* 7*s* 6*d.*

No great historical figure—not even Tallyrand—stands in such need of a complete and critical biography as does the subject of this book. But as with the pupil so with the master and father of diplomacy, the very size of the subject and complexity of the character; the many baffling rôles—bishop and prince of Benevento, angel of light and prince of the air—played by each of them, have discouraged the historian. It requires something like omniscience to write about such super-beings and so far as the Devil is concerned we have to recall the caveat of Samuel Butler, that God has written all the books. This pleasantry conveys of course a very serious fact. Granted (and this in itself is an assumption which we really have no right to make), that there is a single consistent personification which we can call the Devil, have we any chance of obtaining first hand accounts of it? Obviously nearly every original document will have come from a hostile source. And this fact leads us back in a vicious circle to the other difficulty already mentioned—the problem as to whether when we have made a whole from fragments and remedied the exaggerations of malice we are left with a single consistent character at all. If the Devil was for most of those who studied him a sink of iniquity, into that sink they would fling all the garbage of their minds. At the very least there must lie in festering layers, choking—if by its nature they may not fill—the bottomless pit, all the projections and suppressions of man's mind, his hatreds and dreads, his sex horrors and his fear nightmares, his terror of his own nature, of his fellows' hostility and malice, of the pursuing cruelty and violence of beast life and the blind cruelty and violence of inanimate nature. Atop of that lie all his death despairs and next his philosophic defeats, as he struggled, and failed, to lay the problem of evil and had to throw out into this cosmic cess-pool all that he could not fit into his ideal.

How can an antiquary approach such a site? Though the lower layers may have ceased to decompose and have become stratified, the upper are still fresh dumps of foetid controversy. If he attempts a cross-section his horizons at once begin to run into one another. It is therefore no disparagement of Mr Thompson to say that he has not been able to recover an unbroken evolutionary series from such deposits. In a book of 168 pages he could hardly have intended to achieve an Origin of such a complicated spiritual Species. He has however a clear hypothesis. It is that the devil, as the medieval world handed him over to the modern, is no philosopher's familiar. He is not the submerged leaden keel with which idealism balances its ship, so that it still rides the waves though crowded to the top-gallant with every stitch of the wind-provoking white sheets of virtue. He is simply the runt of that fecund family, the fertility gods. His horns are horns of plenty, his hooves are the hooves of fat cattle. He has grown lean and bitter in old age as his merry-making was called sin. An austere heaven has drawn off man's devotion. A harsh dualism has left him only the dirty work. But it was not so in the beginning. Once man's whole life was one, and heaven and hell and this world lay about him, perfectly mingled in his tribal infancy. It was a world of terror, for Mr Thompson starts off as so many religions have started, in a cave; and the first portrait we are given, it need scarcely be said, is the so called wizard from Ariège; but it was a world of wonder, too. The epipalaeolithic and the neolithic are the next stepping-stones on which we are supposed to be able to trace in their highly conventionalized drawings the print of the same person—a wizard who is a fertility god. Again we take a leap and come upon a name—Cernunnos. He is certainly horned and probably a god of death; *ergo* he is the apostolic successor of the palaeolithic dancer. Another leap and we are with the devil we

more or less know, the personage lit by the fires of the Inquisition. Looking back over this immense range of time, can we say yet there are not three persons but one ? Certainly, new factors, which completely transformed the conception, came in at the third stage. Whether the first fertility magic had anything to do with a cult of the dead we shall never be sure ; but it is quite certain that between Cernunnos and Diabolus completely new and elaborately philosophical conceptions of dualism have been introduced, so that the god or gods of the underworld have taken on a nature which compels any classificatory system to separate them from their earlier form. The devil of the witches had undoubtedly a long native ancestry, but the indigenous stock had been made into a new variety by cross-breeding with oriental importations.

In short this book is a stimulating essay asking a question, and though there seem insufficient reasons to conclude that the answer will be in the affirmative, the hypothesis certainly allows many interesting facts to be brought into line. GERALD HEARD.

BURIED TREASURES OF CHINESE TURKESTAN. *By* ALBERT VON LE COQ. *Translated by* ANNA BARWELL. *Allen & Unwin.* 1929. *pp.* 180 *with sketch map, and* 52 *plates.* 18*s.*

This book is a semi-popular account of the second and third German expeditions to the Turfan region (1904–5 and 1905–7). Its German title, *Auf Hellas Spuren*—' On the Tracks of the Hellenistic'—plainly betrays the author's main preoccupation. It is possible that in his insistence on Greek influences he underestimates the part played by the native art of India (Madhura school) in determining the course of Buddhist art, and also the part played by native Chinese painting in forming the styles of Central Asia. New finds are continually making it apparent that the pictorial art even of the first century A.D. in China was already highly developed, and to derive the whole of Buddhist art in China and Japan from the one Indian school of Gandhara is no longer possible. But this insistence on Hellenistic influence has its justification, above all, because it deals with facts. We know what late Greek art is like and can recognize imitations of it. About early Chinese pictorial art we had till recently no trustworthy information. Iranian art obviously played an important part in Central Asia, and now that Herzfeldt has published his frescoes from Samarra it is no longer an eccentricity (as it used to be thought in the case of Professor Strzygowski) to speak of a ' Sassanian landscape '. But at the time of Dr Le Coq's explorations Sassanian painting was still a subject of mere conjecture. He did well, then, to lay emphasis upon a source of influence that could be sifted and checked. The second expedition worked at Turfan and Hami ; the third, at Kucha, Karashahr, Turfan and Hami. The results are now to be seen in the Museum für Völkerkunde, Berlin, where whole temples and façades are reconstructed in the most imposing manner. Among the great accessions to knowledge that Dr Le Coq's labours have brought about none has aroused wider interest than his discovery of exquisitely illuminated Manichean manuscripts, some of which are here illustrated. The conception of Mani as an aesthete has been regarded as a piece of romantic mythology ; now it has found unexpected support.

It must not be supposed that the book is all archaeology. Indeed, behind the narrative, which is always lively and entertaining, lies (partly concealed) the drama of a highly dramatic conflict. Dr Le Coq does not say much about his relations with that strange character Dr Grünwedel ; but there is much that can be read between the lines. It is clear at any rate that the British Museum owes its Central Asiatic treasures not only to Sir Aurel Stein's energy, but also to the dawdling of Grünwedel. A. WALEY.

ANTIQUITY

PI-TZU-WO. PREHISTORIC SITES BY THE RIVER PI-LIU-HO, SOUTH MANCHURIA. *Far Eastern Archaeological Society, Tokyo and Kyoto,* 1929. *Pagination not consecutive ; English text about* 30 *pages, Japanese text about* 80 *pages, with* 68 *plates, some in colour. Price not stated.*

This is the first volume of a series entitled *Archaeologia Orientalis.* It contains the results of excavations undertaken by the well-known archaeologist Kosaku Hamada, assisted by Yoshihito Harada and other colleagues, both Chinese and Japanese, in 1927, at sites near Pi-tzu-wo, close to the Pi-liu-ho river which separates the Japanese and Chinese administrations of the Liao-tung peninsula. The reader will turn with excitement to plates XXV–XXXII, reproducing two whole pots and thirty fragments in the neolithic polychrome style. They show white and red ornament on a blue ground, thus agreeing with the previously-found Manchurian wares (from Hamacho near Dairen and Daitaisan near Port Arthur), but differing from Andersson's finds in Fengtien, Honan and Kansu, where black ornament is generally applied to a red ground. At this Tan-to-tzu site, from which the polychrome ware came, only one piece of bronze was found, lying near the surface. At a neighbouring site, Kao-li-chai, alongside of implements in bone and stone, were found objects in bronze and also in iron. This site is dated, by the finding of Chinese coins, at about 200 B.C. A piece of cast-iron from the site is the subject of a short appendix by Dr D. Saito. There is another excursus (in German) on the human remains, which the authors find to stand in near relation to the modern Chinese. At the later site (200 B.C.) a piece of glass was found (plate XXXVI, fig. 13). Glass objects have indeed been published purporting to come from the Yin-hsu site (12th century B.C.) in Honan, but their provenance has never been substantiated.

The date of the earlier site (from which the painted pottery comes) is put by Dr Hamada at about 400 B.C., perhaps the latest date as yet assigned to objects of this sort. The volume, in short, is of the highest interest, and should not be missed by any one desirous of keeping abreast with the archaeology of the Far East. A. WALEY.

THE TRAVELS AND SETTLEMENTS OF EARLY MAN. *By* T. S. FOSTER. *Benn.* 1929. *pp.* 319. 21*s.*

This book will certainly fall between two schools. For in the first place it is an attempt, similar to Mr Dawson's *Age of the Gods*, though with a wider range, to give a consistent account of prehistory. The actual material is of course still utterly fragmentary ; on the other hand the fragments have now become so numerous that a provisional ordering must be attempted. If no more than for purposes of memorizing we must have some system. But in Mr Foster's painstaking attempt it soon becomes apparent that something is present other than the desire to give an ordered account of all finds. These are so scattered and need, even for their provisional dating, such confirmatory evidence that those which are still controversial should be marked as such ; otherwise a completely wrong scale may be given. Mr Foster however has a thesis to prove. It begins to emerge when man's simian ancestry is being discussed. Indeed Mr Foster's conclusion seems to be a variant of the Preacher's. He would say, ' God made man upright but he sought out many archaic specialisations '. Next Calaveras man is accepted as mid-Pliocene. The Trenton and Lansing skulls are accepted as pre-Chellean and Chellean. In Europe evidences which have been taken by most researchers to point to cannibalism are said to show fragmentation burial. As we proceed, this tendency increases and leads to doctrine even more precarious. English archaeology and Chinese phonetics are strained

indefensibly, until we are forced to recognize that it is a dogma—and that dogma the Diffusionist—that is dictating the value to be given to the facts.

Those therefore who are looking for a critical account of prehistory will be offended. An immense amount of reading has obviously gone to this book but we are given no references, unless a bibliography which cannot represent a tithe of the author's reading can be adjudged such. However controversial the conclusions, they might at least be stimulating if the actual finds on which the author bases his conclusions had been stated. If the book was meant for popular reading and a thorough system of foot-notes would have been out of place, then at least we may plead for a great many more 'mays' or the use of that admirable practice of the Authorised Version whereby, when meaning has to be made from the obscurity of the Hebrew original, the elucidatory word, inserted tentatively by human reason, is put in italics. As it is, it is to be feared that this book will be rejected by many who would have welcomed a text-book of prehistory.

On the other hand there is little reason to suppose that it will prove any more acceptable to the small and emphatic school of dogmatic diffusionism. It certainly seems to smell as much of heresy towards their faith as to the orthodox prehistorian it must show signs of tendencious credulity. The evolution of Diffusionism will one day make an interesting miniature study in the antiquarianism of archaeology. The theory began as crude and definite as verbal inspiration. Since then it has been slowly becoming broader. Still it is doubtful whether this book can be accepted by the faith. For Mr Foster's doctrine is the Anatolian Hypothesis, the name of his eighth chapter, but which might have been the title of the book. For his real interest and main purpose in writing this volume is to maintain that a diffusion of Anatolian stock accounts for the whole of civilization. But even with the Anatolian hypothesis Diffusionism is not made much more amenable to the facts. The advance across the Pacific still remains a problem. For example, to say that because the Easter Island statues probably wore a head-dress—which is quite true—and had their eyeballs inlaid and so had and did the statues from Mohenjo-Daro, that there is thus ground for associating Easter Island with the cities of the Indus Valley, is an association which would link up every culture not under the inhibition of the Second Commandment; for no two sculptural types could be more different than the florid convex naturalistic profile of the one and the archaic, stylized concave profile of the other. If we try a northern route the hypothesis that the Chinese are also products of Sumerian diffusion is no longer admissible.

Indeed it would seem that in this volume we have the unsuccessful fusion of two books—a text-book ordered and uncontroversial, and a contribution to a controversy with which the world of organized studies has no power to deal until the controversialists present a united report. It is to be hoped that Mr Foster will take his book apart, send the thesis to its private address, and give the text-book to the public. GERALD HEARD.

A TOPOGRAPHICAL DICTIONARY OF ANCIENT ROME. *By* SAMUEL BALL PLATNER, *completed and revised by* THOMAS ASHBY. *Oxford University Press.* 1929. *pp.* XXIII, 608, 58 *blocks printed as* 29 *plates;* 7 *text figures and plan.* 35*s*, *or interleaved*, 42*s*.

The late Prof. Platner, whose *Topography and Monuments of Ancient Rome* appeared in 1904 and 1911 had long meditated a topographical dictionary of Ancient Rome, and had invited the collaboration of Dr Thomas Ashby before the outbreak of the war. That interruption materially affected the progress of the undertaking. Dr Ashby's share

had to stand over. Prof. Platner, who was rejected for military service, devoted himself to the task with diligent labour, and was able to sail for Europe in August 1921 with the main part of the book accomplished, and with the hope that a few months of joint work would bring the book to completion.

A sudden and fatal illness overtook Prof. Platner on the voyage, and the finishing of the work was entrusted by Mrs Platner to Dr Ashby. The task has been protracted, and Dr Ashby has been obliged to work in much additional material, as well as to contribute important articles (*e.g.* on the Forum and the Palatine) and to edit and revise the whole for the press. His share in the work is estimated by himself ' at from 20 to 25 per cent '.

In its general form and structure the book suggests a comparison with the *Lexique de Topographie Romaine* of Léon Homo (1900). If the two works are studied side by side it is apparent how much the later work has the advantage of its predecessor, not merely as being a generation later, but also in its wealth of reference (especially to the inscriptions) its careful topographical discussions, and its well chosen illustrations. It is to be regretted however that the plan of Rome, which is taken from the earlier book, is too small for comfortable reading without a magnifying glass. It might have been made a third larger without trespassing on the margin.

Such a work needs constant annotation and revision, and interleaved copies are issued for the use of those who may wish to contribute to a later edition, or it may be merely to keep their own references up to date.

The plan of the dictionary has its limitations, to which Dr Ashby calls attention in his preface. It is undoubtedly a drawback that buildings and sites cannot be found under the names current in the Middle Ages or in the older antiquaries, and that others cannot be included for lack of a name that would make a dictionary heading. It may be hoped that some way of dealing with these matters will be found in a later edition. Appended to the Dictionary is a valuable chronological index to datable monuments, by Dr Gilbert Bagnani. It runs from the dedication of the temple of Jupiter Capitolinus in 509 B.C., to the dedication of the Pantheon as a church 1117 years after.

It would add to the interest of the volume, and not materially increase its bulk, if the inscriptions decisive of topographical questions were given in facsimile. It is not every reader that has the epigraphic materials within easy reach, and can look up a C.I.L. reference.

A. HAMILTON SMITH.

PROBLEMS OF PLACE-NAME STUDY. *By* A. MAWER. *Cambridge University Press.* 1929. *pp.* 140. 6*s.*

This little book consists of three chapters which were delivered as lectures at King's College, London. They are entitled Racial Settlement, the Vocabulary of our Forefathers, and Lines of Interpretation. Professor Mawer gives us here a general survey of some of the more important conclusions which have emerged from the work of the English Place-name Survey. As usual he states them in language which is intelligible to the ' general public ' ; but this does not mean that it is not learned and original in the best sense of both those misused terms.

The book is too short and condensed to summarize, and its contents are just of the kind that one who has closely followed the progress of the Survey might expect. The value of this re-presentation, however, was brought home to the reviewer when he read, on pages 2 to 5, what the author says about the survival of Celtic place-names. In the six counties already published, the names of undoubted Celtic origin are in each case less

than so in number, out of hundreds enumerated. In Sussex, ' beyond a stray river or two, we have no certain Celtic place-names at all '. These facts tell strongly in favour of the ' clean sweep ' hypothesis ; but we think it right to utter a word of caution. With the exception of Sussex not one of these counties was at all thickly inhabited during the Romano-British period. Some, such as Worcestershire, were almost uninhabited. Consequently, there may never have existed any well-rooted Celtic names for many of the natural features ; and such as did exist, being known amongst so small a number of people, would naturally have little chance of surviving a century of turmoil and devastation. Some such explanation surely must be adduced to account for the ' surprisingly small ' Celtic element found in the place-names of Devon—a county for which, we are glad to hear, the material being collected by the Survey is almost complete. Devon was thinly inhabited during the Romano-British period, to judge from the distribution of remains of that period. The adjacent county of Dorset on the other hand was densely inhabited ; and we should expect on purely archaeological grounds to find a higher *proportion* of Celtic names in Dorset than in Devon. Time and the Survey will tell us ; meanwhile, appetites will be stimulated by Professor Mawer's *hors d'oeuvre ;* and the Society itself should gain new members to support its splendid work.

PRIMITIVE BELIEFS IN THE NORTHEAST OF SCOTLAND. *By* J. M. McPherson, B.D. *Longmans.* 1929. *pp.* xii, 310. 12*s* 6*d.*

This valuable study of the primitive beliefs and customs prevailing in the northeast of Scotland during the last three centuries should receive a hearty welcome from anthropologists and students of folk-lore. Precious evidence has been gathered from living witnesses, and from ecclesiastical and burghal records that illustrated the manner in which the Church dealt with the perpetrators of superstitious practices, while a bibliography of seven pages shows the extent of the author's literary researches. His outlook is scientific and he gives a vivid presentation of his material in a minimum of words. Sufficient comparative lore is adduced to enhance the value of the local rites and beliefs, and an occasional clear summary of the evidence adds to the worth of a volume in which no space is wasted on theorizing.

The book is divided into two parts, the first dealing with survivals of nature worship, the second with the Black Art. There are chapters on fire worship and sacrifices, the making of ' needfire ', the cult of holy wells and of the spirits of water, trees, stones, caves and corn. Records of the fairies, the home rites, and devil worship, including the lingering devotion to old gods in the dedication of the Goodman's Croft, lead on to the grimmer evidence of the second part.

Witchcraft was a fully organized cult still operative in the 16th and 17th centuries ; individual witches continued their practices long after that time. The last execution is believed to have taken place in 1722 and the Act repealing the penal law against witches was passed in 1736. The ritual of the witches and their master is reconstructed largely from the actual confessions of initiates during their trials. Their conventions, methods of working weal and woe, transformations and divination, are fully described, as well as the safeguards employed against their ill-will and the fearsome punishments meted out to those who came before the courts.

The concluding chapter deals with the Horseman's Word, the Miller's Word, and the post-Reformation fusion of the fairies with the witch-world, ending with a comparison of the direct and simple character of nature-worship with the baneful intermediarism of the witch-priesthood, the way of which was for ever against the sun.

Few of the records have direct bearing upon archaeological problems. Pages 78–83 give instances of the cult of stones and of their recent erection to bring luck : in marking boundaries a quantity of ashes of burnt wood are first laid on the ground, then big stones are placed above them. It may be suggested that, in a second edition, references to stones might be more fully indexed.

One misprint should be noted : on page 58, Acton *Burnell*, in Shropshire, is designated Acton *Barnett*. The story quoted relates to a dripping well (in Ruckley parish) beside the Devil's Causeway, an ancient road of supposed Roman origin : it was first published by the late Miss C. S. Burne in her *Shropshire Folk-Lore*, 1883, pp. 415–6.

L. F. CHITTY.

DIE DATIERUNG DER ERSTEN DYNASTIE VON UR. *Von* Dr CHRISTIAN. *Zeitschrift für Assyriologie*, NF (*new series*), IV, *June* 1929. *pp.* 233–42.*

The problem of the oldest chronology of ancient Mesopotamia is one of long standing. Of recent years discussion has been stimulated by fresh evidence from three main sources—from cuneiform tablets recording the succession of dynasties from the remotest epoch, and from the results of the excavations at Ur and at the adjacent site of Tell el Obeid. We may state at once that today there is a tendency to reduce considerably the great age formerly attributed to Mesopotamian antiquity. The ' long ' chronology, which placed the beginnings of history in these lands at about 5000 years before Christ, has been succeeded by a ' short ' chronology which places it at about 3100 B.C. German archaeologists now put forward, for the most part, an 'extra-short' chronology. It is this system which Dr Christian attempts to justify in the article under review. His conclusions might be acceptable in themselves if his premises were not susceptible of a different interpretation, and if one had not also to reckon with the civilizations of adjoining countries.

The excavations of El Obeid have revealed to us a group of monuments, notably the remains of a temple, which inscriptions attribute to King Annipadda, of the first dynasty of Ur. This dynasty can only be dated by comparison with the monuments brought to light in the excavations of Tello ; the date of these has been approximately fixed by dead reckoning backwards from a known point in history, and by cross references occurring in later documents.

Dr Christian compares the inscriptions of El Obeid with those of Tello which are most archaic in point of language and writing ; he finds amongst them certain traits which he equates with similar ones in documents of the earliest kings of Tello. From an archaeological point of view, similar facts emerge ; ' plano-convex ' bricks, like those of El Obeid, flat on one side and convex on the other, are found also at Tello where they belong to the earliest reigns (Ur-Nina, Akurgal, Eannatum, etc.). Some of the images and scenes figured at El Obeid in copper and shell can be equated with the oldest at Tello. There is thus a general correspondence in the evidence from the two sites.[1] Finally, the discovery at Ur of monuments containing the names of the kings of Tello (Eannatum and Entemena) shows that the first dynasty of Ur was superseded by these dynasties of Tello, and that it began therefore slightly, but only slightly, earlier than Ur–Nina.

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* Review translated by the EDITOR.

[1] I have already stated these conclusions in ' Les tombes royales d'Our ', *Mercure de France*, 15 August 1928; ' Les tombes royales d'Our et l'histoire de l'art ', *Gazette des Beaux Arts*, June 1929, pp. 321 ff. Mr C. J. Gadd too had determined the relative chronology of El Obeid, the first dynasty of Ur and the archaic period of Tello, by means of a critical analysis of the inscriptions, in *Ur Excavations*, vol. I (Al 'Ubaid) 1927, pp. 125 ff.

But what is the chronological position of all these historical facts? We possess king-lists of the old dynasties of Tello, giving the length of their reigns; there were nine kings from Ur-Nina to Urukagina, and the fifth was actually the grandson of the first, a fact which gives us a clue to the duration of this line of kings. Then comes Lugalzaggisi, of the city of Umma, who captured Tello and who was himself dethroned by Sargon, the founder of the dynasty of Agade, which lasted for 181 years. After that came a dynasty of Uruk, of 30 years; then the Guti, barbarians from the northeast, held the country for 125 years; and when they had been expelled by the liberator Utu-hegal, (who reigned for seven years), there began the third dynasty of Ur, founded by Ur-Nammu. All this is based on the short chronology, according to which the third dynasty is dated 2474–2358 B.C. This short chronology is calculated from astronomical observations recorded by the Mestopotamians themselves, relating to the slightly earlier dynasty of Hammurabi—the first dynasty of Babylon. Recently Dr Kugler, the astronomer, and Dr Weidner, the Assyriologist, have agreed to place the accession of Ur–Nammu at 2300 B.C.[2] It is from this point that Dr Christian starts; following certain documents, he considers that the conquest of the Guti took place contemporaneously with the dynasty of Agade, which was then already restricted to part of central Mesopotamia; while the dynasty of Uruk reigned at the same time (and not subsequently) over southern Mesopotamia. Dr Christian thus produces the following table of events :—

Ur–Nammu - - - - - - - - - -	2300
From the end of the dynasty of Agade to Ur–Nammu (including the Guti and the dynasty of Uruk) - -	100
From Annipadda to the end of the dynasty of Agade (including the domination of Ur by Tello, a second dynasty of Ur, and the dynasty of Agade) - - - - - -	180

Since these figures have to be added to the date accepted, according to the new revision, as a starting point, we get, for the beginning of the first dynasty of Ur, the period 2580–2500 B.C. To this date are assigned also the Royal Tombs of Ur, which have yielded stylistically contemporary objects. There is an important difference in the dating, for the usual short chronology gives the following figures :—Ur–Nammu 2474; end of the dynasty of Agade 2649; Ur–Nina, about 3000 or 2900. This would put the first dynasty of Ur and the Royal Tombs at 3000 or 3100 B.C.

What is one to think about these new figures? In themselves they seem to allow a sufficient lapse of time for the historic incidents concerned; they should however be received with caution, for several reasons. The first is that the reckoning of the aforesaid astronomical facts is subject to variations. The revised estimate of the astronomer Dr Fotheringham and the interpretations of the documents by Dr. C. Schoch both nearly confirm the figures of the short chronology. The second reason is that this 'extra-short' chronology does not take account of neighbouring civilizations. It is difficult to explain how Egyptian civilization, whose beginning, even if we adopt the lowest estimates, is placed by Egyptologists at about 3200 B.C., should have preceded the beginnings of history in Mesopotamia by 600 years. The intercourse between Asia and Egypt in early historic times is an accepted fact; it would become inexplicable, as also

[2] V. Christian and E. F. Weidner, *Das alter der Gräberfunde aus Ur*. Archiv für Orientforschung, V, 1929, pp. 139–150.

would the development of Minoan civilization, if we adopted these new figures. Were the offerings made by the Pharaohs of the First Dynasty to the temple at Byblos intended for a colony situated in a land of barbarians? Were the expeditions despatched by Sneferu to obtain cedar-wood from Syria sent to a still uncivilized land? The suggestion seems impossible to accept. I think therefore that Dr Christian's most interesting and learned studies reach a conclusion which can only be accepted if similar researches should lower, by the same amount, the date of the beginnings of civilization in Egypt and the Aegean.

G. CONTENAU.

CURIOSITÀ ITALICHE DI STORIA, ARTE E FOLKLORE I. LEO MONTECCHI. Nemi, il suo lago, le sue navi. *Rome: Luciano Morpurgo.* 1929. *pp.* 123, *with* 32 *plates, and* 20 *text illustrations.* 9 *lire.*

This little book is the first of a collection of small volumes which it is proposed to publish, dealing either with individual sites or the local customs of a particular district; in future it is to be under the direction of Professor Giuseppe Lugli.

The present volume is certainly not as good as it might be, though it contains some good and interesting illustrations and a certain amount of information. A systematic bibliography would have been useful, and might have taken the place of the conjecture that Caligula built the ships in order to prosecute his amours with Diana! I cannot myself see, as a fact, why the statements that we have that the name of Tiberius was read on one of the pipes should be rejected, while the bronze grille with the inscription CAISAR which was found by Fusconi in 1828 must, one would think, belong to the time of Claudius. As to the nature of the ships, which were simply floating platforms (though the first, now uncovered, has the shape of a ship, but with a very broad beam), our author is more or less right, except that the presence of lead pipes inscribed with the names of Tiberius and of Caligula shows that there was a permanent water-supply from the land to the ships, and that they differed from houseboats or college barges at Oxford in that they could not be moved about even on occasions. It is absurd to say that this idea is to be banished to the realms of fable and that these pipes were only waterpipes for the water from the ships (p. 72). *What* water? one may well ask. Nor is he very happy in other matters. Prof. Lugli has already pointed out that the temple of Diana at Nemi, like a number of the other great sanctuaries at Latium (the temple of Jupiter Latiaris among them), consisted of a large temenos with a number of *small* buildings in it: so that we are not here to imagine the existence of a hexastyle temple of the usual classical type on a podium 30 metres by 16 (p. 33).

Nor is the road leading down to the lake from Genzano to be identified with the Clivus Aricinus or Clivus Virbius (not Virbis). This is, rather, the steep ascent of the Via Appia from the ancient post-station of Aricia towards Genzano (p. 39). And the statement on the next page that a second road 'must have diverged from the magnificent Via Sacra, discovered in the last few years, which leads from Ariccia to Monte Cavo, and which, passing by Fontana Tempesta, and reaching to the edge of the crater, must have descended steeply to the lake' does not convince me. The banks of the lake at this point are too steep even for that. Finally, let us close with the hope that a local museum will *not* be built on the spot. If the remains of the ships are worth preserving *in situ* (supposing that, as Comm. Biagini remarks on p. 112, the first is sufficiently well-preserved to make it worth while to uncover the second), this must of course be done. Otherwise, the less alteration of the landscape and the less building that is done, the better, and things should, as far as possible, be allowed to return to their previous aspect.

Careful studies were made in 1892* with regard to the question of the supply of water to the lake of Nemi by springs which rise in the interior of the lake basin itself, whether above or below the water level. Of the former class there are two springs, which produce a total of 63 litres per second ; while the sub-aqueous springs were calculated as adding 119 litres or nearly twice as much again. So that some 182 litres per second, apart from any help from rainfall, will be available for filling the lake when it has been drained as far as it is desired to drain it. The springs of the larger lake of Albano on the other hand only supply 105 litres per second, the rest coming from the rainfall. T. ASHBY.

ON ALEXANDER'S TRACK TO THE INDUS ; personal narrative of explorations on the north-west frontier of India, carried out under orders of H.M. Indian Government. *By* SIR AUREL STEIN, K.C.I.E. *Macmillan.* 1929. *pp.* XVI, 182, *with* 97 *illustrations*, 2 *maps.* 21*s.*

Sir Aurel Stein left Delhi in February 1925. His first aim was to explore the Swat Valley and its ruined Buddhist shrines. The Swat district is the Udyana of Buddhist legend, the fertile ' garden ' famous for its miracles and piety. ' There are memorials of the Buddha wherever one goes and always a stupa or temple to honour them ', says Huisheng, a sixth century Chinese pilgrim. Many of the stupas, in various degrees of decay, still survive ; on fig. 13 Sir Aurel shows one at Amluk-dara, which he describes as the most perfect existing specimen in India. He was able to identify numerous other famous ' sights ' described by a succession of Chinese pilgrims—Buddha's foot-print, the rock where he dried his clothes, the Stone Couches mentioned by Hsuant-sang. This latter pilgrim arrived in Swat in the second quarter of the seventh century, about a hundred years after the catastrophic invasions of the White Huns, and he found Buddhism in a state of comparative decay. But the Korean pilgrim Hui-ch'ao, whose *Travels* were discovered by Professor Pelliot at Tun-huang (Chinese Turkestan) and published too recently to be utilized by Sir Aurel, gives an interesting account of the great Buddhist revival that occurred less than a century later. ' The people ', says Hui-ch'ao, ' had given up most of their land to the monasteries, and of what they retained a considerable part was set aside for the purpose of supplying the monks with food and clothing. There were more monks in Udyana than laymen '. We thus see that the extreme religiosity of modern Tibet continues a Himalayan tradition.

The main object of Sir Aurel's expedition, as indicated by the title of his book, concerned a far earlier period—that of Alexander's Indian campaigns. The Greek texts have never before been handled by any one with adequate local knowledge. Sir Aurel's identifications are epoch-making. Above all, his correlation of Una with Aornos and of Pir-sar with the famous stronghold captured by Alexander from the Assakenoi are noteworthy feats in the realms of both philology and geography. A third object of the expedition was linguistic research. This is dealt with in a very cursory way. Very few readers have any idea what is meant by a ' Dard language ' ; and if such terms are used at all, it is better to define them. The illustrations, maps and index are excellent, and Sir Aurel shows throughout that interest in actualities and human situations which makes all his travel-books so different from mere records of archaeological discovery. A. WALEY.

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* Carta Idrograpghica d'Italia. Le acque sotterrane dei colli laziali. Reprinted from vol. 12. Rome, 1892, pp. 28–49.

ORIENTAL INSTITUTE OF THE UNIVERSITY OF CHICAGO. First Report of the Prehistoric Survey Expedition. *By* K. S. SANDFORD *and* W. J. ARKELL. *University of Chicago Press.* 1928. *pp.* 52, *sketch-map and* 29 *illustrations. Price not stated.*

Dr Sandford and Mr Arkell spent several months in the early part of 1927 in travelling by motor-car, camel, railway and steam-launch in the Egyptian portion of the Nile Valley and by motor-car across the Eastern Desert to Koseir and back, for the purpose of studying the river-terraces of the Nile Valley and Red Sea coast and their contained relics of prehistoric man. This first report does not profess to be a detailed scientific account of the work accomplished by the authors, but is essentially a semi-popular exposition, designed for the educated general reader, of the general nature of their investigations and the views they have formed on the geological history of the Nile Valley and its occupation by early man.

A short introductory chapter, in which the importance of stratigraphical studies in connexion with the finding of stone implements is emphasized, is followed by a brief outline, in very simple language, of the geological history of Egypt, including the first formation of the Nile Valley (which the authors regard as having been eroded to its maximum depth long before the advent of man), its conversion by regional subsidence into a gulf or arm of the sea extending as far inland as Esna, the infilling of the gulf for a very considerable proportion of its depth by deposited sediments, and the re-elevation of the land with consequent regression of the sea from the gulf and re-conversion of the latter into a river-valley. Then follows a chapter dealing with the later stages of the valley's history, including the erosion of the gulf-deposits by the river, and the formation, owing to alternations of climate or to successive earth-movements (both are mentioned as causes), of five terraces at successively lower levels, only the last four of which, ranging from about 30 metres to about 4 metres above the present Nile-level, have been found to contain relics of prehistoric man. The fourth chapter treats of the succession of human industries in Europe and Egypt, and here the implements of the four lowest Nile-terraces are classed, from above downwards, as of Chellean, Acheulian, earliest Mousterian, and Mousterian types respectively. In a final chapter is given a brief narrative of the movements and adventures of the Expedition.

The report is admirably clear and readable in style ; and in view of the avowed purpose for which it was written, one ought not, perhaps, to cavil at an almost entire lack of any allusion by the authors to the work of the many investigators who have preceded them, nor at the omission of even brief accounts of the evidences on which many of the statements in the report are based. But it seems a pity that the old myth of an abandoned Nile-bed existing in the Libyan Desert should have been resurrected (on p. 6) ; and it is with regret that we infer, from a sentence on p. 44, that the heights of the various terraces were estimated by the comparatively rough method of aneroid-observations, rather than by a more exact procedure, such as could easily have been followed in the Nile Valley with its abundance of precisely levelled bench-marks.

Professor Breasted, through whose efforts the necessary funds for the Expedition were granted, and who contributes a preface to the report, is to be congratulated on having secured from Oxford two such well-qualified and enthusiastic field-workers as Dr Sandford and Mr Arkell. Their first report can be recommended for perusal by all who are interested in the prehistory of Egypt ; and a fuller and more technical account of their work would be cordially welcomed by geologists and archaeologists alike. JOHN BALL.

REVIEWS

THE MAORI PAST AND PRESENT. *By* T. E. DONNE, C.M.G. *Seeley Service and Co. Ltd.* 1927. *pp.* 287, *with* 46 *illustrations and map.* 21*s.*

As a Government official Captain Donne held many responsible appointments in New Zealand, and is well qualified through his long acquaintance with the native people and their language to give an instructive and sympathetic picture of their mode of life. In this book, while making no pretence to a very deep scientific treatment of his subject, he has yet managed to cover a wide range of topics in interesting fashion, and to treat some of them, as wood carving and the working of jade, from a novel angle. His remarks on the transition of Maori culture from the primitive to the 'civilized' state merit attention, though he sets perhaps too high a value upon the attempt to assimilate the native completely to European standards.

At some points there is a tendency to over-emphasize the more sensational features of the old-time Maori life—to represent it, rather unfairly, as 'a place where there existed neither legal nor moral restraint, where might was right, murder a pastime and cannibalism a cult' (p. 208). A more adequate treatment of the whole subject would have laid stress on the less picturesque but more fundamental matters of work and the management of property, while analysis of these in relation to the social structure, the grouping into families and tribes, the kinship system and the position held by the chiefs would leave the reader with a less confused idea of the native law and morality. The reproduction of dialogue in Maori 'pigeon English' (*sic*) is to be deprecated, and is at times anachronistic, as when introduced into the story of the launching of the schooner *Herald* in 1826; the use of such a plural as *karakias* is unnecessary, the *s* being a European accretion. Apart from these and certain other minor errors the book offers an entertaining if not too abstruse description of Maori custom.

RAYMOND FIRTH.

EXPLORATIONS IN HITTITE ASIA MINOR: A PRELIMINARY REPORT. *By* H. H. VON DER OSTEN. *Oriental Institute Communications* no. 2. *University of Chicago Press.* 1927. *pp.* VIII, 104 *with* 101 *figs.*

EXPLORATIONS IN HITTITE ASIA MINOR, 1927–28. *Oriental Institute Communications no.* 6. *University of Chicago Press.* 1929. *pp.* 153 *with* 160 *figs.* 4*s* 6*d.*

The extraordinary importance of Asia Minor as the ancient seat of a peculiar and powerful civilization, and also, as we now begin to realize, as the main intermediary through which the achievements of ancient oriental culture were handed on to Greece, stands in striking contrast with our astonishing ignorance of its monuments and other remains. Time and again, in the study of pre-Hellenic history, one has to confess that further progress is impossible or that conclusions cannot be vindicated, because no evidence from Anatolia is available. But there is, at last, a fair chance that this gap in our knowledge will be largely filled. The Oriental Institute of the University of Chicago has resolved to back efficaciously the enthusiastic author of the above named publications, and is enabling him to carry through a five-years campaign in three directions: exploration, survey, and excavation.

These results of the work will be finally embodied in larger publications, while the *Communications* now under review are described as 'popular illustrated reports of Institute projects in the form of preliminary bulletins for general readers', and they fulfil this purpose admirably. The first starts with a survey of the rôle of Asia Minor in history, and then describes in a lively way the author's first voyage of exploration. His descriptions of landscape, together with the numerous illustrations, give one a vivid

impression of the land of the Hittites in its physical peculiarity. The military antecedents of the author suggest to him interpretations of certain constructions, *e.g.* as watch towers, tumuli, and signal stations which would not, perhaps, have occurred to others, but which may happen to be correct in the case of the Hittites, a racial minority uniting under their rule a country by nature unfit for unity.

The second volume gives an account of the further work, with an enlarged staff, including the extremely important mapping of certain areas with the exact notation of all ancient remains. Furthermore a number of newly discovered monuments are already published here, while the results of the excavation of Alishar, undertaken to obtain a sequence of pottery fixed by stratigraphical observations, will soon be published in full and are here only summarized. Those who heard the author's lecture on this subject at the archaeological congress in Berlin last spring, will regret that the general reader, for whom the *Communications* are intended, does not receive this story in a somewhat fuller form. For it was most instructive—an exemplification as well as a vindication of archaeological methods—to note how the relative chronology of the wares, such as I tried to establish some years ago on the basis of intrinsic (stylistic) evidence and of evidence from surrounding countries, was confirmed by the excavations, which, on the other hand, enabled the explorers to attain a precision, and a richness of detail, which only careful work in the field can produce.

The difficulties which explorers have to face become apparent on almost every page. The author and his collaborators are to be congratulated on the courage with which they carried on, and the Oriental Institute on the farsightedness which induced it to use its organization and its resources for this novel project. H. FRANKFORT.

I. EPIGRAPHIC SURVEY OF THE GREAT TEMPLE OF MEDINET HABU. *By* HAROLD H. NELSON. II. THE ARCHITECTURAL SURVEY. *By* UVO HOELSCHER. (Seasons 1924–28). *Oriental Institute Communication no.* 5. *University of Chicago Press.* 1929. *pp.* XIV, 50, *with illustrations and* 3 *plans.*

The great organization known as Chicago House, which stands between the desert and the sown behind the Colossi of Memnon at Thebes, has issued this bulletin, which gives in some fifty pages the history and aims of the Expedition together with a very clear explanation of its technical methods of work. Everything that money could do has been done to equip and staff the building and library which house the Expedition and to eliminate both human and mechanical error in the records made. Perhaps no piece of archaeological work was ever planned on so generous a scale

The first few pages of the communication deal with the historical significance of the events recorded on the walls of the temple of Rameses III. Mr Nelson emphasizes the importance of this period when the older civilization of the Aegean and of Greece itself was being destroyed by the northern invaders, the ancestors of the Greeks of History. The artistic value of the historical scenes once covered with plaster and colour is noticed. Professor Hoelscher then describes in part II the architectural investigation of the temple and its annexes. The Expedition is fortunate in having secured the services of Professor Hoelscher, whose knowledge of Egyptian architecture is so great, as his reconstruction of the two palaces of Rameses III prove. On page 39 mention is made of *poisoned* arrowheads of bone and flint. Was it possible to establish after so long that these arrowheads were once really poisoned ? Also were not the groves in the sandstone walls (cf. photograph on p. 17) made by sharpening spears or arrows of copper and iron ? Arrowheads of bone or flint must have fallen into disuse before the time of Rameses III, except perhaps for sporting purposes. F.M.C.

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